AN OCEANIC QUEST

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THE INTERNATIONAL DECADE OF OCEAN EXPLORATION

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An Appraisal by Committees of the
NATIONAL ACADEMY OF SCIENCES
AND THE
L ACADEMY OF ENGINEERING

AN OCEANIC QUEST

THE INTERNATIONAL DECADE OF OCEAN EXPLORATION

Prepared under the auspices of the

Committee on Oceanography
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and the

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PREFACE

On March 8, 1968, the President of the United States of America proposed the launching of "an historic and unprecedented adventure—an International Decade of Ocean Exploration for the 1970's." The general concept was described in a report of the National Council on Marine Resources and Engineering Development published in May of 1968. The Council then invited the National Academy of Sciences (NAS) and the National Academy of Engineering (NAE) jointly to provide advice on the scientific and engineering aspects of United States participation in such a Decade of Exploration. The Academies agreed to examine the scientific and engineering goals and priorities among these goals, the capabilities required to achieve them, the program elements of a Decade of Ocean Exploration, and the end products and benefits to be anticipated if the Decade were to be implemented. The subsequent study and the preparation of this report were financed by the National Council on Marine Resources and Engineering Development.

A Steering Committee for the Decade study, with Warren S. Wooster as chairman and William E. Shoupp as vice chairman, was organized by the NAS Committee on Oceanography (John C. Calhoun, Chairman) and the NAE Committee on Ocean Engineering (Thomas C. Kavanagh, Chairman). At the first Steering Committee meeting on August 7, 1968, a preliminary outline of Decade goals was developed, a set of fundamental criteria to identify programs appropriate for the Decade was established, and a workshop to bring together a larger group of scientists and engineers to examine in detail the pertinent matters for definition of the framework and substance of a Decade of Ocean Exploration was planned.

At the workshop held at Woods Hole, Massachusetts, on September 4–13, 1968, some fifty specialists, nominated by the NAS Committee on Oceanography and the NAE Committee on Ocean Engineering, worked together to identify programs of exploration effort that could contribute to enhancing utilization of the ocean. These programs were discussed, organized, and reorganized and submitted to the Steering Committee. During this process many programs were considered, but only those that, in the judgment of the members of the working group, were of primary importance and met the agreed-upon criteria and objectives were retained. A list of Steering Committee members and workshop participants is given in the Appendix.

During the following few months, the Steering Committee continued to develop and refine the criteria and the proposed programs for the Decade exploration effort. Based on the Committee's judgment, this report proposes for initial consideration a number of programs that are germane to Decade goals and objectives.

During the development of this report, drafts were reviewed in detail by the Committee on Oceanography, the Committee on Ocean Engineering, all participants of the Woods Hole Workshop, government agency representatives, representatives of the NAE Council, a representative of the Earth Sciences Division of the National Research Council, and selected scientists and engineers who had not participated in the preparation of the report. These reviews have been most helpful and have been used in final preparation of the report.

While generous use has been made of the drafts prepared at the workshop and of the advice and comments from reviewers, the final determination of criteria, emphasis, and the programs selected for inclusion was made by the Steering Committee. Furthermore, although the Committee on Oceanography and the Committee on Ocean Engineering have added their endorsements, this report is based on the judgment and decisions of the Steering Committee.

The conclusions and recommendations should be viewed as the basis for further detailed planning and, of course, should not necessarily be interpreted as those of the National Marine Council. This report does not imply any future action by the United States Government.

Further development and consideration of the concept, programs, and implementation of an International Decade of Ocean Exploration will now require more-detailed planning and budgeting by groups in both the governmental and nongovernmental sectors. An International Decade of Ocean Exploration could well be included within the broad scope of commitment by the United States to the oceans envisioned by the Commission on Marine Science, Engineering, and Resources in its recent report *Our Nation and the Sea.*

As the concept of an International Decade of Ocean Exploration continues to be developed in the United States and among the ocean-oriented nations, new programs of merit will emerge and changes in emphasis may be required for scientific, engineering, political, and financial reasons. This, of course, is as it should be. During this process we hope that this report, *An Oceanic Quest,* can serve as a useful basis for discussion and planning both nationally and internationally.

CONTENTS

PROLOGUE

The origins of scientific inquiry can be traced back at least to the flowering of Greek culture more than 2,000 years ago. Throughout the intervening millenia, the ocean, its boundaries, and its inhabitants have been the objects of scientific interest and investigation. Yet the great age of geographical exploration did not reach its climax until late in the eighteenth century, and intensive and systematic oceanographic research began only in 1873 with the departure of HMS *Challenger* on her global exploration. During the past century, and particularly during the past few decades, scientific knowledge of the ocean has accumulated at an ever-increasing rate.

The use of the ocean and its resources is of even more ancient origin. Subsistence fishing has been practiced for at least 10,000 years, and as civilization spread out from its sources adjacent to the China Sea, the Arabian Sea, and the Mediterranean Sea, man has used the surface waters for transport and for communication with his neighbors. As civilization became more complex, the ocean was used in more complex ways; for example, only recently it has become a major source of mineral resources and a basic element in the forecasting of global weather.

The marriage of ocean science and ocean use has been a fairly recent development. Only in the last few years have the maritime nations fully recognized the potential value of ocean resources and the need for providing an adequate foundation of scientific understanding. This recognition comes at a time of growing aspirations for a better life and when the needs of an expanding world population for new sources of food, raw material, and fuel are becoming critical.

During the past decade, marine science has been pursued with the principal goal of gaining fundamental understanding of ocean processes. Although this goal will never be fully achieved, important progress has been made. We consider it appropriate that in the coming decade emphasis be given to the goals of prediction and of enhanced and rational utilization. In subsequent years, accumulated knowledge should ultimately lead to the enlightened and responsible stewardship of our ocean heritage.

An International Decade of Ocean Exploration has been proposed for the 1970's to give new impetus to those studies that will enable man to realize more effectively the promise of the sea. This report examines the possible scientific and engineering content of such a Decade, particularly with regard to U.S. participation, and considers the potential benefits resulting therefrom. At the same time, some thought has been given to the capabilities required and the constraints to be overcome in order to achieve the desired goals.

DISTINGUISHING FEATURES OF THE DECADE

The term "International Decade of Ocean Exploration" can be interpreted very broadly. Thus the Steering Committee gave early consideration to the features that could serve to distinguish programs of the Decade from the whole of ocean science and engineering. A broad statement of the basic objectives of the Decade was developed, as follows:

To achieve more comprehensive knowledge of ocean characteristics and their changes and more profound understanding of oceanic processes for the purpose of more effective utilization of the ocean and its resources.

The emphasis on utilization was considered of primary importance. In contrast to the total spectrum of oceanography and ocean engineering, the principal focus of Decade activities would be on exploration effort in support of such objectives as (a) increased net yield from ocean resources, (b) prediction and enhanced control of natural phenomena, and (c) improved quality of the marine environment. Thus Decade investigations should be identifiably relevant to some aspect of ocean utilization.

The word "exploration" has a number of meanings, extending from broad reconnaissance to detailed prospecting. Exploration effort of the IDOE should include the scientific and engineering research and development required to improve the description of the ocean, its boundaries, and its contents, and to understand the processes that have led to the observed conditions and that may cause further changes in those conditions.

Of all the ocean investigations that will contribute in some way to enhanced utilization, we believe that those involving cooperation among investigators in this country and abroad are particularly appropriate for the Decade. Decade programs would often be of long-term and continuing nature, would require the facilities of several groups, and would be directed toward objectives of widespread, rather than local or special, interest. It is anticipated that these programs within the United States may be cooperatively implemented both by government agencies (federal and state) and by private facilities (academic and industrial).

As the title suggests, international cooperation will be of particular importance. Such cooperation has long been a characteristic of oceanography, for reasons described in the following paragraph (from "International Ocean Affairs" published by the Scientific Committee on Oceanic Research in 1967).

> The world ocean covers 71% of the earth's surface. Most countries have sea coasts and make some use of the sea, although national jurisdiction extends over only a small fraction of the ocean's area; the remainder is common property.* The waters of the world ocean and their contents intermingle without serious restraint. Many oceanic processes are of large scale and are driven by forces of planetary dimension. The organisms inhabiting the sea are influenced by these processes and forces, and their distribution, abundance and behaviour are often influenced by events occurring far beyond the territorial limits recognized by man.

Most international cooperative investigations have consisted of a set of national programs suitably modified and coordinated to achieve international objectives. The Decade is envisioned as a period of intensified collaborative planning, development of national capabilities, and execution of national and international programs. This report gives principal attention to the development of U.S. programs that could contribute to the Decade. Integration of these programs and those of

* Or belongs to no one—Ed.

other countries into a comprehensive international program was not discussed in detail, but has been left for consideration by appropriate international bodies. It is hoped that this report will be a useful contribution to those discussions.

In the light of the goals and features discussed above, there appear to be important aspects of ocean research and development that lie outside the framework of the Decade. For example, some aspects of theoretical and experimental research, or the development and application of specific exploitation techniques, may not be appropriate. Some oceanographic research of an academic nature and certain mission-oriented programs of government and industry will not fit logically into the Decade. For example, the National Council on Marine Resources and Engineering Development has estimated that only about 30 percent of the present U.S. federal marine science budget (as defined by the Council) is designated for programs related to ocean exploration. In a sense, all investigations in the ocean will contribute to the goals of the Decade, but in order for it to be successful, a definite set of programs must be determined. The distinguishing features discussed above should help in defining this set.

The term "Decade" can be understood in a general way to mean the 1970's. Inception of the programs must await completion of planning and the availability of adequate facilities and funds. Formal completion of the Decade might be scheduled for early in the 1980's. Achievement of the long-term goals may require continuing investigations and adjustments in specific programs during or following the Decade as the effect of these investigations on economic and social uses becomes apparent.

USES OF THE OCEAN

Among the ways in which man uses the ocean, the following activities should be included:

Use of living resources; use of mineral resources (including production of oil, gas,* and freshwater); shipping and navigation; establishment and protection of coastal works; siting and maintenance of cables, pipelines, and tunnels; disposal of wastes; forecasting of oceanic and atmospheric conditions; warnings and forecasting of storm surges and tsunamis; extraction of tidal and thermal energy; recreation; and national and collective security.

Each of these activities can benefit, to a greater or lesser extent, from

* For simplicity we include oil and gas among the "mineral" resources though strict use of this term includes only the inorganic materials.

the results of appropriate investigations envisioned for the Decade. In the long run, standards of living should rise with the greater availability of protein foodstuffs at lower costs throughout the world. The aggregate supply of energy-producing resources will be greater as a result of offshore production. Other resources, both mineral and organic, presumably lie on the continental shelves and in the deep ocean; geological and geophysical reconnaissance is necessary for the development of orderly programs of detailed exploration and exploitation. A basis of scientific and engineering information is required for conservation and management and for international agreements dealing with the ocean and its resources.

Increased use of the ocean and its resources may tend to exacerbate the already existing potential for conflict among maritime nations. Such conflicts usually cannot be resolved exclusively on technical grounds. Yet there is a significant component of a technical nature. For example, fishing disputes frequently arise from lack of biological knowledge of the resource being exploited. Jurisdictional disputes over the resources of the sea floor may be due in part to inadequate scientific and engineering information. It is hoped that Decade programs will make an important contribution to the diminution of international tensions as they relate to ocean problems.

With regard to both the extractive and the nonextractive uses of the ocean, Decade investigations should result in improved prediction of environmental conditions and may lead toward eventual modification or at least limited control of these conditions. Better forecasts can reduce losses of life and property, permit more effective planning, and increase the efficiency and convenience of operations at sea. An understanding of the consequences of intervention in the marine environment should reduce deleterious effects or facilitate exploitation of potentially beneficial effects.

Despite their focus on utilization, the objectives of the Decade are related to exploration and knowledge rather than to the development of techniques for the large-scale exploitation of ocean resources. From an economic point of view, application of this knowledge should provide a basis for greater output, lower costs, and improvement in the organization of production and use. Anticipated benefits are long-term in nature, and justification of the Decade goes beyond immediate economic returns.

It should be recognized that there are legal, economic, and social aspects to enhanced utilization of the ocean and that these aspects must also be investigated if the benefits of the Decade are to be attained. Therefore, appropriate proposals of this sort are included in this report.

OBJECTIVES OF NATIONAL PARTICIPATION IN THE DECADE

The objectives of any nation participating in the Decade could be summarized as follows:

1. To benefit directly the growth of the national economy
2. To obtain information required for management and conservation of resources, for improving the effectiveness of nonextractive uses; for prediction, control, and improvement of the marine environment; and for the making of sound political, legal, and socioeconomic decisions related thereto
3. To provide the technical basis for the reduction of international conflicts in the ocean
4. To benefit directly the economies and populations of developing countries
5. To increase knowledge and understanding of the ocean
6. To expand the technical resource base (manpower, facilities, and technology) for future ocean research and utilization

The United States is already extensively engaged in the development of ocean resources, both in local waters and in many other parts of the world ocean. U.S. private interests are investing large sums in exploration and drilling for oil, in capital and labor in the fisheries, in coastal development, in marine transportation, and in other uses of the ocean. The government is also incurring large expenses in connection with utilization of the ocean and its resources. At the same time, significant revenues are accruing as a result of these activities. Over the past 20 years, income to the U.S. Treasury collected as bonuses, rentals, and royalties on offshore oil and gas leases exceeded $3 billion. Royalties alone in 1968 were nearly $200 million. Large amounts were also paid to several coastal states. Investigations such as those proposed for the Decade are necessary for the rationalization, protection, and extension of investment opportunities for capital both off our own coasts and elsewhere in the world.

ORGANIZATION OF THE REPORT

The following section, "Summary and Major Recommendations," is intended to stand alone as a complete synopsis of the report. The arrangement is keyed to uses of the ocean and its resources.

In the section entitled "Programs," the discussion is centered on four major topics: geology and nonliving resources, biology and living resources, physics and environmental forecasting, and geochemistry and environmental change. Under each of these topics, both scientific and engineering aspects are considered. Representative projects are discussed as examples of possible Decade activities as envisioned at present; the insights, interests, and priorities of the ocean community are continually evolving, and it is certain that the eventual Decade programs will differ to some extent from those discussed in this section.

In the final section, "Ways and Means," the capabilities required for achieving Decade goals are discussed, along with the pertinent constraints to be overcome. Financial, organizational, political, and technical considerations are reviewed, and attention is drawn to their implications for successful prosecution of the Decade.

SUMMARY AND MAJOR RECOMMENDATIONS

OBJECTIVES, GOALS, AND CHARACTERISTICS

We propose the following basic objective for the Decade:

To achieve more comprehensive knowledge of ocean characteristics and their changes and more profound understanding of oceanic processes for the purpose of more effective utilization of the ocean and its resources.

As a corollary to this objective, the following set of goals should be adopted:

To acquire by 1980 an enhanced capability to

- Exploit, conserve, and manage in a rational, economic manner the major living resources of the ocean, and the major nonliving resources of the continental margin
- Evaluate realistically the economic potential of the nonliving resources of the deep-sea floor and provide the factual basis for rational decisions about their jurisdiction

• Make useful predictions of oceanic conditions on operationally significant time scales

• Control modifications of the marine environment resulting from man's intervention

• Operate effectively at the surface, within, and at the bottom of the ocean

Programs appropriate to the Decade would, for the most part, be *long-term* and *continuing* investigations of *cooperative* nature, directed toward *objectives of widespread interest* concerned with more-effective *utilization* of the ocean and its resources.

The principal emphasis of the Decade is on the use of the ocean and its resources. A noteworthy outcome of the discussions among scientists and engineers was the consensus that more effective utilization is now importantly limited by lack of technical information, understanding, and capability. An exploration effort was considered the appropriate and desirable activity for the large-scale cooperative programs of the Decade. The more local and intensive prospecting and development of exploitation techniques, on the other hand, is a task for the parties directly concerned.

It is possible to identify relatively specific goals in several fields of marine affairs. It is more difficult to specify detailed plans for future research. An inherent characteristic of research is the inability to predict what will be the most fertile lines of attack on identified problems for several years ahead. Therefore, we have proposed for initial consideration a number of programs, described in more or less detail, that are germane to the Decade objective and goals. These examples are drawn from our present experience and understanding; because of the continual evolution of this understanding, it is probable that other proposals of higher priority will subsequently arise.

RELATION BETWEEN OCEAN USES AND DECADE PROGRAMS

In the program chapters of this report, the rationale is presented for investigations in the program areas of geology-geophysics, biology, physics, and geochemistry. Each chapter includes a number of representative programs of both scientific and engineering interest. These programs are outlined in a subsequent summary list.

In view of the proposed objective and goals of the Decade, it is also useful to examine some important uses of the ocean and their potential benefit from investigations of the type proposed. The following topics have been selected for discussion in the present section: mineral resources, living resources, waste disposal, and ocean transportation. Other uses are not analyzed in the same manner, but in general, they will benefit from the same types of investigations. Additional uses of the ocean may be revealed by the Decade exploration effort.

Mineral Resources

At present, exploitation of marine mineral resources* is essentially confined to the continental shelf. The principal product is petroleum and natural gas. In 1967, the sea floor adjacent to the United States was the source of about $1.7 billion worth of petroleum, natural gas, and sulfur, about four times the production in 1960. U.S. offshore production is about one third of total world offshore production. Other shelf and near-shore resources include sand and gravel, tin, gold and platinum, hematite, magnetite, ilmenite, light heavy minerals such as rutile, zircon and monazite, and diamonds.

In addition to the resources currently being exploited, other potential resources include phosphorite on the shelf and upper slopes, and manganese nodules on the deep ocean floor. It is also known that petroleum in some regions is present beyond the shelf, although it is not being exploited by present techniques. Information is needed on the abundance, composition, and distribution of deep-sea deposits, for an evaluation of their utility and as a basis for management and jurisdictional decisions.

A variety of scientific and engineering investigations is required to expedite the use of ocean mineral resources. Physiographic mapping and reconnaissance geological-geophysical exploration of the continental margin can provide the basis for subsequent intensive study and prospecting by industry. Delineation of the continental margin and the transition to the deep ocean is required as an element in ultimate establishment of regimes and jurisdiction. Exploration of small ocean basins and the deep-sea floor can facilitate evaluation of mineral-resource potential. Fundamental studies of sea-floor structure, sedimentation, and processes affecting these will help in reasoning about little-known areas. The development of mineral-recovery systems will require knowledge and prediction of surface oceanic and atmospheric conditions as well as sea bed characteristics. Ecological studies are required for guidance in the control of pollution from mineral-recovery operations.

* Including oil and gas.

Living Resources

In 1967, more than 50 million tons of organisms were harvested from the ocean, with a dockside value of about $8 billion. The world catch has been doubling in about ten years. It has been estimated that the sustainable yield of conventional living resources is four to five times the present harvest. This amount is equivalent to the total requirements for animal protein of the 6 billion people expected to be living by the year 2000.

United States landings in 1967 were 2.4 million tons, little different from the catch thirty years ago. At present, about 70–75 percent of the fishery products used in the U.S. are imported; much of the deficit between consumption and production might be made good from underused resources already known to be present off the coasts of the United States if it were not for a number of institutional constraints.

Apart from the institutional factors that tend to limit the growth of marine fisheries in the United States and elsewhere, there are a number of technical constraints that could be reduced as a result of appropriate investigations. In order to exploit unused resources, maximize the net yield, reduce the cost of production, and conserve and manage the stocks in an effective manner, it is necessary to understand the factors that control the abundance, distribution, and availability of fish populations of commercial interest. Some of the basic studies required are ecological in character and concern the transfer of energy from the sun and atmosphere through the various levels of the food web. The dynamics of exploited populations and their ecological associates must be analyzed. In addition, exploration of the locations, sizes, and changes of fish populations, studies of oceanic processes that lead to usable concentrations of fish, and research on fish behavior, are necessary. The operations of fishing vessels will also benefit from the investigations specified below for ocean transportation.

Waste Disposal

An important use of the ocean is as a receiver for the waste products of our civilization—sewage, heat, chemical wastes, dredging spoil, and so on. Both deliberate disposal and inadvertent discharge (as of pesticides and oily wastes) are steadily increasing. At the same time, the ancient assumption that the ocean has an infinite capacity to absorb such wastes has already been proved wrong in several instances.

Not all waste discharges are necessarily harmful, but most probably are and thus can be called pollutants. Their deleterious effects include harm to living resources, hazards to human health, hindrance to maritime activities including fishing, and reduction of amenities. It is conceivable that some discharges, such as heat and sewage, could be so introduced as to produce beneficial effects, such as increasing primary production. Pollution-control technology has reached the point where the nature of many effluents discharged to the environment can be controlled, if the cost of this treatment can be met.

If man wishes to control those modifications of the marine environment resulting from his activities and use them to his own advantage, he requires several kinds of oceanographic information. Depending on their physical and chemical nature, introduced substances may be subjected to advection and diffusion, to adsorption on particles, to settling out or exchange at the bottom, and to absorption, concentration, or transfer through the food web. These processes are important in many other aspects of marine research and utilization. Thus their study is pertinent to a variety of Decade goals. In addition to studies of such processes, it is also essential to establish present concentration levels as a base line from which future changes can be measured.

OCEAN TRANSPORTATION

The ocean is the major coastal and international highway for the transport of heavy and bulky materials. By 1975, it is estimated that the annual world ocean freight bill could be as large as $15 billion, of which the United States will pay about one third. To carry this freight, there were in 1966 a total of 25,620 vessels larger than 300 gross tons (with 1,810 more under construction); the United States operated about 9 percent of these. At any given time, about two thirds of these ships can be expected to be at sea.

Merchant shipping endeavors to deliver cargoes on schedule, as rapidly and cheaply as possible, and with maximum safety to personnel, vessel, and cargo. Many of the problems in achieving these goals of speed, economy, and safety are of institutional or socioeconomic character; others are indirectly related to oceanographic knowledge (such as the design of ships and its dependence on surface ocean conditions), and some are directly responsive to understanding, prediction, or control of oceanic and atmospheric conditions.

Approximately a third of the ocean freight bill is incurred during the transfer of cargo across the ocean–land interface. The development of new methods for cargo transfer, more-efficient maneuvering of large ships in confined waterways, improved charting, the design of better harbors, control of silting, and control of pollution can be assisted by oceanographic studies of bathymetry, of the basic structural nature of the sea floor, and of waves, currents, and associated mixing processes.

Investigations of the sort proposed for the Decade can also contribute to reduction of the sea-borne portion of the ocean shipping bill. The design of more-efficient surface ships will be enhanced by more-comprehensive knowledge of the statistics of surface waves. Radical designs, including submersible freighters and surface effect vessels, will also benefit from oceanic knowledge. Through better prediction of surface ocean and atmospheric weather, optimal routing of ships will reduce fuel consumption and time at sea, and diminish the danger of storm losses. Stranding and collision losses can be minimized with better coastal navigation and charts.

SUMMARY LIST OF PROGRAMS

From each of the program chapters, specific program proposals have been extracted and are presented in outline form below. Items in the first two chapters are directly relevant to mineral resources and living resources, respectively. In other cases, relevance to the uses discussed above is indicated by the symbols MR (mineral resources), LR (living resources), WD (waste disposal), and OT (ocean transportation).

Geology and Nonliving Resources

Continental Margins

1. International cooperative reconnaissance of the emerged and submerged continental shelf of the eastern margin of the Atlantic, from northern Norway to the Cape of Good Hope, with continuous measurement of seismic, magnetic, and gravity parameters, and with bottom sampling and coring, along lines spaced at 50-mile intervals.
2. International cooperative geological-geophysical surveys of the contiguous shelves and slopes of different countries.
3. Assistance to coastal states in detailed hydrographic surveys in nearshore waters and harbors (OT).
4. Cooperative hydrographic survey and charting of the continental margins.

Small Ocean Basins

5. Geological-geophysical investigations of selected basins (Mediterranean, East Indies, Red Sea) for assessment of mineral-resource po-

tential, particularly of petroleum, and elucidation of tectonics and sediment dynamics. Work should include bottom sampling and seismic reflection and refraction measurements.

6. Continuation of the deep-sea drilling program, with emphasis on small ocean basins and continental margins.

Ridges and Trenches

7. On mid-ocean ridges (especially the Mid-Atlantic Ridge), geological and geophysical studies involving precise navigation and hard-rock sampling capability with manned and unmanned devices and surveys for hydrothermal deposits (i.e., metal-rich brines).

8. Studies of a trench at a continental margin (Peru-Chile Trench), with dredging and coring at sea and sampling on land, geophysical profiles both at sea and ashore, and detailed earthquake seismology studies of submarine earthquakes using land-based seismometers.

Deep Sea

9. Systematic, coarse-scale surveys of the deep ocean to provide basis for more-intensive reconnaissance of resources. Broad-scale reconnaissance geophysical surveys and deep coring to yield regional sediment description.

10. In the South Pacific, survey selected sites for manganese nodules and phosphorite deposits to ascertain their distribution and composition.

11. Extend magnetic coverage, using airborne magnetometers; improve global picture of gravity, with coordinated program for operating gravimeters on ships of opportunity and for compilation of past and incoming results.

Biology and Living Resources

U.S. Fisheries

1. To explore, and to assess the production potential of the numerous latent living resources in the Gulf of Mexico and Caribbean Sea, and in the Gulf of Alaska, using all available assessment techniques, including those presently under development.

2. To explore the stocks of oceanic tuna and tuna-like fish, particularly skipjack, in the Equatorial Eastern and Central Pacific and to devise suitable means for their utilization.

3. To investigate and describe the interactions in the great multi-species, multi-nation fisheries of the northwestern Atlantic, thus providing the scientific basis for the establishment of management policies for these fisheries.

International Fisheries

4. To explore and assess the production potential of the oil sardine, mackerel, shrimp, and other fisheries stocks of the Arabian Sea, and to promote participation in their increased utilization by the countries bordering this sea.

5. To investigate the resource potential of krill and the notothenid fishes of the Antarctic Ocean, to devise means for their extraction and for channeling the products into the world's fish economy.

6. In cooperation with local governments, assess the fishery resources of southern Chile and Argentina, especially those in the semiprotected fjords, where local industries might be encouraged.

7. In cooperation with local governments, explore and assess the fishery resources of the continental shelf of the Indonesian archipelago, in its wide sense, especially with regard to stocks of demersal fishes and prawns.

Other Biological Studies

8. To apply existing analytical models of plankton production to available data from a few well-studied areas of the world ocean and to use these models or improved ones to guide the design of observational programs.

9. To apply recently developed techniques to the study of food chains in the sea, and to develop new techniques of measuring biological parameters, especially in a small number of areas selected for their wide relevance as models of typical situations (WD); the suggested places are Georges and Grand Banks, the Gulf of Alaska, the Gulf of Mexico, the Eastern and Central Equatorial Pacific, the South Pacific gyre, the western Arabian Sea, and the Antarctic Ocean.

PHYSICS AND ENVIRONMENTAL PREDICTION

Monitoring (OT, LR)

1. Extend the use of selected ships of opportunity and aircraft for collection of near-surface oceanographic data. Encourage the establishment in developing countries of simple shore stations of standard design.

2. Establish more permanent ship and island midocean monitoring stations, including several heavily instrumented island observatories.

3. Investigate the requirements for design of an effective system for oceanographic monitoring of the North Pacific.

4. Support pilot studies of new monitoring techniques, such as free-fall transport measuring devices, moored current-meter arrays,

deep-sea bottom-pressure recorders, and air-dropped expendable and retrievable instrument packages.

Air–Sea Interaction (OT, LR)

5. In the Western Indian Ocean, investigate reaction of the ocean to monsoonal changes in winds, using an existing numerical model to design an observational program.
6. In the Western Pacific and China Seas, use existing data to construct a preliminary numerical model.
7. In the Equatorial Pacific, conduct an observational program using research vessels and buoys, ships of opportunity, aircraft and island stations to elucidate large-scale, long-term ocean–atmosphere interaction.
8. Select a subtropical upwelling region for the investigation of mesoscale interaction.

Deep Ocean

9. Complete world coverage of deep-water temperature, salinity, and dissolved-oxygen measurements. Obtain direct measurements of deep-water flow in selected locations (WD).

GEOCHEMISTRY AND ENVIRONMENTAL CHANGE

1. Conduct a geochemical survey of selected chemical and radiochemical substances on meridional traverses in the Atlantic, Pacific, and Indian Oceans (MR, WD, LR).
2. Monitor the rate at which natural and man-made substances are being added to the ocean by rivers and winds (LR, WD).

GEOGRAPHICAL REGIONS OF GENERAL INTEREST

Although the program chapters were developed more-or-less independently, there are certain geographical regions that appear to be of general interest. The coincidence of interest is most pronounced in the case of physical and biological programs. This may reflect the mutual interest in time-dependent variables and the important interactions between the circulation and the biosphere. It is also true that physical and biological studies are somewhat more compatible in the field than either is with a geological-geophysical program, so that data coverage tends to be somewhat similar for the two fields.

Of the various regions where interests in biology, fisheries, physical oceanography, and meteorology appear to coincide, two are selected for discussion here. One is the vast region of the Equatorial and adjacent South Pacific; the other is the more restricted region of the Arabian Sea.

In the Equatorial and South Pacific, large-scale and long-period ocean–atmosphere interactions are of great importance. Variations in atmospheric and ocean circulation are closely interrelated and may have profound biological consequences through the accompanying changes in vertical circulation on and near the equator. The use of ships of opportunity, aircraft, weather buoys, island stations, and special cruises is proposed in order to monitor the equatorial system and to provide data with which numerical models can be designed and tested. South of the limits of the equatorial zone lies one of the most data-deficient parts of the world ocean, and attention is drawn to the desirability of completing the coverage of high-quality deep oceanographic stations there. The processes of enrichment in tropical waters are of particular interest to biology and fisheries, as are the opportunities for studying quasi-steady-state plankton communities in the South Pacific gyre. The Tropical Pacific is the home of valuable living resources, including the skipjack tuna, so that region is suggested as one of particular importance to the U.S. fisheries.

The Arabian Sea is dominated by the reversing monsoon wind system, making it a region of great scientific interest where improved understanding of the atmosphere–ocean interactions could lead to forecasting methods with tremendous social and economic consequences. An existing preliminary numerical model can be used in the design of the most appropriate monitoring network. The monsoonal response of the ocean circulation along the western boundary, including the dramatic development of the Somali jet current during the southwest monsoon, provides an opportunity to decipher problems of considerable theoretical importance. Vertical circulation induced along the boundaries results in an extremely productive regime, particularly during the southwest monsoon. Thus the region is a valuable model for low-latitude biological systems with intermittent upwelling, which if better understood could lead to the rational use of a greatly enhanced production of the living resources that are urgently needed for the nutrition of this highly populated region.

In both of these areas, we have proposed that resident ships be incorporated in the observational programs. Local research facilities are modest, and resident ships could provide the means to make the necessary physical and biological measurements over suitably long periods of time. They could serve as economical platforms for visiting investigators with interest in the regions, and could provide valuable training and experience for local scientists, engineers, and technicians.

SUMMARY OF RECOMMENDATIONS ON IMPLEMENTATION

The final chapter of this report is concerned with ways and means of achieving Decade goals. Details of implementation and logistics must be elaborated by planning and administrative staff, both national and international. The recommendations summarized below are intended to suggest a framework in which realistic Decade programs can be developed.

It should be noted that during the preparation of this report, many more programs were proposed than have finally been included. This selection of programs constitutes a first rough priority judgment of those that should initially be supported. With further planning and more extensive consultation among the scientific and engineering communities, other programs worthy of support will emerge.

FUNDING

Significant additional funds over those now being spent will be required for upgrading present facilities, for developing new capabilities in preparation for Decade programs, for conducting fieldwork, and for analyzing and publishing Decade results. Some elements, such as facilities and specialized manpower, require several years of lead time. If the Decade is to be implemented on a significant scale by the mid-1970's, funds for these purposes will be required in the near future. *If in the United States adequate additional funds are not made available, both laboratories and government agencies will be forced to reprogram to meet Decade obligations, to the probable disadvantage of their essential regular activities.* In this case, it would be undesirable to identify the set of new programs as an International Decade of Ocean Exploration.

Gross estimates of limits and patterns of expenditure are given in the body of the report. During the first three years of the Decade, emphasis should be on planning and on the upgrading of present facilities. The development of new capabilities should begin no later than the second year, and the major implementation of programs, as they are presented in this report, should be initiated in the third year. By the middle of the Decade, some of these programs will have been replaced by new proposals. Some programs could be started in the first year.

National Capabilities

Manpower

Any significant expansion of national activities in ocean exploration will increase the demands for manpower in this field. To some extent, these enhanced manpower requirements can be met by transfer from related fields. However, there will continue to be a requirement for an increasing number of professional marine scientists and engineers. Because of the long time required for specialized training, manpower requirements resulting from Decade activities should be kept under continuing review so that action to increase the supply can be taken as necessary and in timely fashion.

Buildings

Despite the vigorous building programs of recent years, many active laboratories are unable to take on new major responsibilities because of space limitations. Academic teaching and research are especially restricted because of shortage of space. An analysis of building requirements should be made at centers where Decade programs are to be conducted, to ensure that these expanded activities can be adequately accommodated.

Platforms

A long-term interagency plan should be developed for analysis of ship requirements of academic laboratories, and funds should be provided for upgrading the academic fleet. The research and survey vessel building program of government agencies should be continued, and funds should be provided for full operation of available ships. Resident ships, analogous to *Eltanin* and *Anton Bruun,* should be operated in certain regions. Funds should be provided to qualified laboratories for charter of commercial vessels, submersibles, and aircraft.

Special Installations

Programs of the National Oceanographic Data Center, the Smithsonian Oceanographic Sorting Center, the National Oceanographic Instrumentation Center, and the Bureau of Standards should be broadened and strengthened as necessary to meet the national needs for the processing, analysis, storage, and retrieval of oceanographic data, and the testing and calibration of instruments and for the provision of standards.

Associated Equipment

Funds should be provided for equipping laboratories, and research vessels, with computers, satellite or other precise navigational systems, autoanalytical equipment, and other expensive tools of modern oceanographic research.

Improved Technology

Significant effort will be required from the beginning of the Decade to achieve the technological advances necessary for its implementation. New capabilities in fields such as the following will be required for the programs of scientific and engineering exploration and research.

Navigation

An adequate global navigational system is required for the successful implementation of many Decade programs. The most promising system appears to be a combination of very-low-frequency (VLF) and satellite navigation. The present system requires further development and wider availability. In addition, high-precision, short-range navigation will be required for work on the continental shelf; where existing systems are adequate, organizational and financial arrangements are needed to make them more generally available.

Platforms

Platforms with new capabilities will be required for some Decade programs, and support should be provided for their necessary development. The utility of a high-speed, surface-effect research vessel and of instrumented aircraft for work in the South Pacific should be explored. Autonomous instrumentation is required for more-effective use of ships of opportunity.

Versatile and reliable moored buoys are required for the successful implementation of various Decade programs, as well as for other national purposes. To achieve this capability, continuing and adequate support should be given to the present national program of research, development, testing, and evaluation of data-buoy systems, which can be of major assistance to many Decade programs.

The development of suitable sensors for measuring oceanographically useful parameters from aircraft and satellites (for example, departure of the sea surface from the geoid, sea-surface roughness, surface temperature even in the presence of cloud cover, and surface chlorophyll) should be expedited. Support should be provided for the design and construction of improved submersibles for a work-and-support mission at moderate depths. Further consideration should be given to the instrumentation of bottom-mounted facilities for studies of air–sea interaction and variability, geophysical measurements, aids to navigation, and for monitoring the ocean and atmosphere. Further development of self-contained instrument capsules and of unmanned, self-propelled probes should be encouraged.

Living Resource Location and Extraction

Improved methods and instruments are required to reduce the unit cost of capture of living resources and to develop the capability for the economic exploitation of presently unused resources. Improved methods of search and detection will also aid in the exploration and assessment of living resources. Particular attention should be given to the perfection of acoustic systems, to extend their range, resolution, and versatility. The development of optical- and chemical-detection systems should also be pursued. Studies of fish behavior and of the response of fish to various physical stimuli are required for the development of new techniques for aggregation and capture of living resources.

Survey Methods

With improved methods, the hydrographic survey of the continental margins and the deep-sea floor could be made less expensive and time-consuming. Emphasis must be given to the development of improved automated survey systems, which might be coupled through shipboard computer systems to all required navigational and environmental information, so that sea-floor charts can be produced quickly and with minimum intervention from personnel.

Data Management

Effective utilization of the anticipated large volume of Decade data will require modern techniques of data management. A common system of sensing, communication, and analysis should be developed for real-time oceanographic and meteorological data. For archived data, present data-exchange systems, both national and international, must be strengthened, modernized, and automated. The development of versatile methods of analysis and display should be accelerated, particularly with regard to the "live atlas," where immediate access to data banks permits the convenient exploration of existing information. Methods must also be developed for the efficient handling of nondigital information, such as geological and biological samples. Special attention must be given to extracting and making available data and information useful for engineering applications.

Data Standards

For ready intercomparison and pooling of data from cooperative projects, there must be further research on improvement of methods and international consultation and agreement on reference methods and standards.

National Program Management

Planning

It should be an early task for a central planning staff to relate the concepts and proposals in this report to the on-going and planned programs of federal, state, and private institutions in order to ensure that Decade-relevant activities are clearly identified and integrated into an over-all program. A mechanism should be established, possibly through the National Academies of Sciences and of Engineering, whereby the ocean science and ocean engineering communities can continue to present their views on possible Decade programs and their modification as the Decade proceeds.

Assignment of priorities must await further elaboration of program details by potential participants. In establishing priorities, the intent should be to eliminate less-desirable projects, not to reduce support of all projects to fit available resources. For most Decade programs, some preliminary distribution of resources among discipline-related areas may first be necessary, after which projects can be ordered on the basis of their relative importance as judged by members of the scientific and engineering communities. For those projects where eventual economic returns can be identified, the criterion of potential economic value can be used in ordering projects and in establishing their relative value to society.

Coordination

Coordination of Decade programs funded by the United States Government should be effected through some appropriate interagency group. A new composite body is required to coordinate federal activities with those of state or private organizations (including industry). In coordination of the over-all U.S. program with those of other nations, the State Department must continue to depend heavily on the other coordinating bodies for technical advice.

Review

External review of Decade programs, for the purpose of augmenting, updating, and evaluating their scientific and engineering aspects, should be provided for, possibly through the joint NAS-NAE group referred to above. It is also desirable continually to monitor and appraise the utilization achievements (as an on-going benefit–cost analysis), the supply and demand for specialized manpower, and other socioeconomic aspects of the progress of the Decade and its impact on society. An independent body may be required for this purpose.

Associated Social Studies

In view of the objective of the Decade of achieving more-effective utilization of the ocean and its resources, and the several goals to increase the net economic yield from the use of marine resources, it is desirable for the scientific and engineering studies of the Decade to be accompanied by investigations in fields such as law, economics, political science, and sociology. Because of the importance of such investigations, their funding and coordination should be incorporated as an integral part of Decade planning and implementation.

Strengthening Possibilities for International Cooperation

Mutual Assistance

The United States can benefit from the assistance of other coastal nations in carrying out or facilitating many Decade programs and from an increased capability of these nations to interpret and apply resource information and to make rational decisions on investment and management of such resources. Cooperative programs of the Decade provide an opportunity for strengthening this capability in a number of developing countries. Provision of required technical assistance, through bilateral and multilateral arrangements and on the basis of a careful analysis of each country's potential and needs, can be an important contribution to achievement of the principal objectives of the Decade.

Freedom of Scientific Research

Recognition should be sought among countries participating in the Decade that the scientific research portion of exploration and reconnaissance is an integral part of fundamental scientific research. Convenient procedures for facilitating cooperative research on the continental shelf and in the contiguous fishing zone are essential. Nations participating in the Decade must find ways to remove barriers to the free exchange of personnel and data and to facilitate the passage of scientific materials through customs and the entry of research vessels into their ports.

International Coordination

Coordination Tasks

During the planning stage, available information must be reviewed, analyzed, and discussed, and agreement must be reached on objectives and methods. Then intergovernmental discussions, both bilateral and multilateral, will lead to agreement on over-all objectives and on the nature and extent of national participation. Coordination during the period of fieldwork depends on good communications and full exchange of information. Monitoring and review of the achievement of scientific, engineering, and technological objectives and of the economic and social consequences of the Decade are necessary. At the conclusion of the field program, coordination and joint action are required in the exchange, analysis, and interpretation of results, as well as in their dissemination.

Existing Organizations

Where possible, existing technical advisory bodies should be used for planning, review, and evaluation of Decade programs. More-effective means must be found for involving engineers and representatives of industry, as well as appropriate social scientists, in this process.

Several intergovernmental organizations with responsibilities related to Decade goals will inevitably be involved in the organization and implementation of the Decade. Of these, the Intergovernmental Oceanographic Commission (IOC) is taking a lead role in coordinating international planning. The capability of IOC to organize and coordinate a program of this magnitude and complexity should be carefully analyzed, and steps should be taken to ensure that it, or possibly another more appropriate body, is given the structure and support required for the task. Regional intergovernmental organizations, such as the International Council for the Exploration of the Sea, will undoubtedly undertake some responsibility for Decade programs of regional character; better machinery than now exists is required for coordination in regions bordered largely by developing countries.

Relation to Other Programs

A number of cooperative international investigations pertaining to the ocean and atmosphere have already been planned for the 1970's. In most cases, the goals of these programs are close to those of the Decade; some of those whose achievement will be facilitated by such action should be absorbed and coordinated as part of the Decade. Others may develop more effectively outside the Decade. In such cases, it is essential that close cooperation and exchange of information be effected and that Decade-relevant aspects be identified and monitored in the manner proposed for Decade activities.

BENEATH THE SEA

Geology, Geophysics, and Nonliving Resources

INTRODUCTION

An International Decade of Ocean Exploration (IDOE) could, as the President noted, "bring closer the day when the people of the world can exploit new sources of minerals and fossil fuels." The earth sciences have been fruitful contributors toward this objective on land, and we anticipate that they will be at sea. Land exploitation began as the essentially accidental field discovery of resources and developed into the present limited capability for predicting where to look for those resources. Similar developments may be expected at sea from the earth science-related programs of the Decade.

We do not believe that the Decade should be expected necessarily to result in the delineation of immediately exploitable resources. Indeed, it would be a serious mistake to saddle it with any such aim. Instead, the objectives should be a broad general survey to provide background for the later detailed investigations of resources and the implementation of carefully selected scientific programs designed to increase basic understanding of the earth and the sea.

There is sufficient experience with programs of this sort on a limited scale to suggest optimism regarding the potential benefits of expanded but related programs during the IDOE. Let us consider a few examples.

Industry has made good use of the reconnaissance investigations of oceanographic institutions, universities, and governmental agencies when such investigations have suggested that conditions are favorable for the development of resources. This has occurred along the Atlantic margin of the east North American coast. Geologic conditions on the adjacent land have not favored the abundant production of petroleum, and as a consequence, there was little interest in this coastal area while other shelves of the United States were being explored and developed. After reconnaissance studies by oceanographers indicated the presence of unsuspected sediments of substantial thickness and broad areal extent, industry began to study this area intensively. This is illustrative of the appropriate role that should be assigned to the exploration and survey phase of the earth science program of the Decade.

Despite the broad general nature of the reconnaissance for nonliving resources proposed above, it is likely that significant contributions toward the discovery of resources will be made. An example is the important show of oil encountered on the Sigsbee Knolls (Gulf of Mexico) during the Deep Sea Drilling Program sponsored by the National Science Foundation.

Modern oil finding is based on a hard-won understanding of the genesis of oil, the structure of the earth, the physical properties of sediments deposited in different environments, and the hydrodynamics of groundwater flow. The importance of basic earth science is widely appreciated in the mineral-resource industries. One reason is the unexpectedly direct relationship between marine geology and the geological history of the highly petroliferous oil province of California. Studies by oceanographers have demonstrated that the deep marine basins off Southern California are exact equivalents of the oil-bearing Ventura and Los Angeles basins, except that they are not yet wholly filled with sediment. Muds rich in organic matter are modern equivalents of the dark shales in which the oil in California originates. Sands deposited in deep water by bottom-seeking suspensions called "turbidity currents" are exactly the same as many of the sands into which the oil migrates and from which it is pumped in oil fields. The gradual understanding of the modern habitat of oil revolutionized the geological interpretation of the adjacent land and, consequently, the geological search for more oil.

Plate tectonics is a quite new, quantitative way of analyzing the deformation of the earth's crust. It has developed during the last year and is based largely on discoveries at sea. The geophysical study of the continents indicates that much of what is now continent was once ocean basin, and it is becoming increasingly evident that some ocean basins were probably continental land masses in the past. Mapping of marine magnetic anomalies in the mouth of the Gulf of California has shown a characteristic symmetrical pattern of anomalies of known age. These confirm that the peninsula of Baja California was adjacent to Mexico to the southeast before the anomaly pattern developed. The mouth of the Gulf of California has opened during only the last four million years. Plate tectonics requires and quantitatively defines, at least in ideal circumstances, the rate and direction of motion of surrounding regions once a measurement of the velocity of any part of a plate is determined. Thus, it now appears that the geology of Southern California has been profoundly affected by the opening of the gulf. The San Andreas fault and related parallel faults have been the locus of more than 100 miles of offsetting of the adjacent continental crust during only a few million years. Once again, specific investigations at sea have had a revolutionary effect on the geological understanding of an important part of the continents. Moreover, the general geological principles discovered at sea apply to the whole earth and thus result in a broader understanding of all of the continents and their resources.

The production of topographic and geological maps of land areas is a common function of most governments. Topographic maps, in particular, are essential for many purposes, including the development of natural resources and the construction and maintenance of transportation systems. Charts are needed for similar purposes at sea, but the available ones provide inadequate detail except in a few small areas. They contain little information on composition of the sea floor and even in previously charted areas may not adequately reveal hazards to navigation, since these can shift position under the influence of currents and waves.

Sea-floor mapping, in both shallow and deep water, is necessary before several of the Decade goals can be met. However, detailed mapping of the entire ocean floor by present methods would take many decades and would require expenditures that may not be economically justified. Therefore, we suggest that during the Decade, mapping should initially be confined to essential areas while a significant effort is directed toward the development of new and more-efficient surveying technology. As new instruments and methods become available, the surveying effort can be broadened appropriately.

As suggested by the new evidence of the origin of the Gulf of California, earth scientists are in the midst of one of the most important revolutions in thinking of this century. This is related to the concept of sea-floor spreading and plate tectonics. No one could have predicted this concept a decade ago, and thus we are justifiably chary of attempting to outline a program of research for the next decade. In the United States the number of oceanography students and the number of scientific papers in oceanography are doubling at four- to five-year intervals. A decade from now, four times as many people will have access to four times as much published information as we have today. They should not be hampered by detailed, and inflexible, plans for future basic research. We believe it is more important now to plan for timely exploitation of new scientific opportunities as they arise and for the continuation and growth of the international cooperation, the environment of scholarly research, the responsive and sensible monetary support, and the educational programs that have made the recent revolution in thinking possible.

In this spirit we have identified several survey and research programs that are closely related to the broad objectives of the IDOE and that we believe merit vigorous support. Some of these will require years of international cooperative effort. Some may seem small and can be accomplished in a year and can then be replaced by other programs, which we do not venture to propose. Even the smaller programs, however, are global in scope, and some are capable of absorbing the efforts of an entire subdiscipline of science. Marine geology and geophysics are growing rapidly, but at present the research capabilities can easily be overwhelmed.

Our understanding of the origin and history of continents and ocean basins includes motions and interchanges, fluxes and alterations, rather than stability. The oceanic crust is created along midocean ridges, moves laterally as a rigid plate, and then plunges into the mantle beneath continental margins or in zones of crustal convergence. The continents are relatively permanent, although they drift about as parts of the rigid oceanic crustal plates. On the other hand, material is constantly eroded from the continents and is deposited in oceanic sediments, or it remains for long periods dissolved in the sea. Some questions that require answers are: What mechanism controls the transformation of continental oceanic areas? What happens to the oceanic crustal material that plunges beneath the continents? Do rivers or winds transport more sediment to the oceans? Was the relative importance the same in the past? Does it vary from place to place? Is sea-floor spreading episodic? Why do continents sometimes drift with the ocean crust and sometimes not? These are the kinds of questions we cannot now answer with any confidence.

We believe that progress toward the solution of many important problems would result from the programs outlined below.

These are specific programs, but they relate into a whole that is nothing less than an understanding of the processes affecting the earth and every living creature on it. We can view this whole in several ways: for example, in terms of the movement of materials, whether desired diamonds or rejected wastes, from the continents and back. In the geochemical sections and the hydrogeochemical inventory discussed elsewhere in this report, we follow the materials from the continents into the oceans and see how they are distributed by oceanic and atmospheric circulation. In the study of continental margins and mediterranean and marginal seas, we see where much of the coarser material is concentrated, including the potentially most valuable mineral resources. By the study of plate tectonics, we can determine if the sediment is smeared back against the continental margins by sea-floor spreading and at what rate. Finally, the processes that deform continental margins and form metalliferous ores may be determined by studies at a suspected mantle convergence. Thus we have reached a point in oceanography where the investigation of one global problem rather clearly and immediately provides data related to many others. Consequently, the rapid and accurate exchange and compilation of information will be one of the most important requirements of the IDOE.

POTENTIAL MINERAL RESOURCES

What is Known

As a preliminary to the development of a rational program for the IDOE, we have reviewed marine mineral resources and their relative importance now and for the next several decades. In an attempt to evaluate the significance of these resources, we have given consideration to reserves on land, present and projected demand, potential problems in discovery and recovery, anticipated effects of alternate materials on exploitation, and the impact of the production of large quantities of a resource upon its market value and upon the economy of small nations. The present produced value of the principal mineral resources is given in Table 1.

These resources are ones for which there is a known possibility of offshore extraction. The abundance and the probability of offshore exploitation of individual minerals is considered briefly below.

TABLE 1 The Present Produced Value of the Principal Mineral Resources [a]

	Annual Value of World Production ($ millions) (estimated 1967–1968 range)	
Resource	Total	Offshore [b]
Petroleum	26,000	3,900
Sulfur	340	37
Sand and gravel	900 (U.S.)	150 (U.S.)
Heavy heavy minerals		
Gold	1,900	*
Tin	460	24
Platinum	150	*
Light heavy minerals		
Ilmenite	54	*
Rutile	16	*
Zircon	10	*
Monazite	1.8	*
Gems		
Diamonds	290	4
Precious coral	2	2
Subsurface consolidated deposits		
Coal	18,500	35
Iron ore	4,300	17

[a] Data from U.S. Bureau of Mines.
[b] Asterisk indicates less than $500,000 excluding beach sands.

Petroleum

Oil and gas (and sulfur, of which the offshore production has been associated so far with oil and gas production) are by far the most valuable minerals presently extracted from the sea floor. Present offshore production is from the continental shelf, but at ever greater depths. Production from slopes and deeper portions of the continental margins is possible, but it may be limited to giant fields. However, even in the face of a continuing and substantial demand for petroleum and the advances of deep-water petroleum technology, the high cost of recovery from the deeper waters and the availability of alternative sources may delay exploitation for decades.

Sand and Gravel

Sand and gravel are being economically recovered offshore but are a poor second to petroleum in value. Offshore sand and gravel production will be confined to the shelf for the foreseeable future.

Heavy Heavy Minerals

Tin, gold, and platinum can be expected to be available for exploitation only on the shelves and near shore. Sea tin has been mined for decades, and further offshore exploratory activity is under way in areas known to be favorable.

Light Heavy Minerals

Ilmenite, rutile, zircon, and monazite can be expected in commercial quantities only on beaches and very near shore. Some of these minerals are being mined.

Gems

Gems, mainly diamonds, can be expected in commercially recoverable concentrations only on shelves mainly near shore. At present diamonds are being recovered commercially.

Precious coral has considerable commercial value and occurs on the shelves and upper slopes.

Manganese Nodules and Phosphorite

Two other oceanic deposits are of potential value. These are manganese nodules and phosphorite. Manganese nodules of varying composition appear to be widespread over the deep ocean floor. Their potential value lies in their nickel and copper content, with subsidiary value for cobalt, chromium, molybdenum, and manganese, in that order. Before the economic significance of these deposits can be determined, more work is needed to establish their compositions and their horizontal and vertical distributions on and in the sea floor. Phosphorite occurs at numerous places on the shelf and upper slopes. Because of the large supplies of high-grade phosphate resources on land, most deposits, while of potential value, will probably not be commercially attractive for some time. Possible exceptions to this could be where significant high-grade deposits are found close enough to a source of high phosphate need (India, for example) to offset higher offshore production costs by lower transportation costs.

Other Resources

Still another possibility is the Red Sea hot-brine deposits. These deposits are intermediate in depth and are potentially valuable for their zinc, copper, and lead content. Even though they are relatively near the shore, their commercial value is probably not to be achieved within this decade.

Other nonliving resources that may prove to be of economic benefit at some time but that are of little significance at present include other deposits like those of the Red Sea, submarine coal and potash, sunken ships, bauxite and gibbsite, and many small-scale deposits, found almost exclusively on shelves.

In view of the above, for the next several decades, it appears that the regions of greatest potential for commercial recovery of geologic resources from the ocean are the continental margins.

IDOE Programs and Projects

Recovery of minerals from beneath the ocean floor requires extensive information in all ocean science disciplines from almost every part of the world. The geologic information needed consists of bathymetric, seismic, gravimetric, and magnetic data, together with bottom samples and cores. In addition, data on the soil mechanics of the ocean floor and physical and chemical data on weather, waves, currents, and the composition, density, clarity, and temperature of water and ice are needed in order to permit engineering design and development of efficient mining and recovery systems. Information on existing biological regimes and their susceptibility to change is needed to establish permissible ranges of change in the environment as a base line for pollution control. Improved weather forecasting and navigational systems will be of great benefit to all ocean-mining and oil-recovery operations.

On the continental margins, project areas can be identified for the purpose of planning and organization. One such area could well be the eastern margin of the Atlantic Ocean from the northern tip of Norway to the Cape of Good Hope. An average spacing of survey lines at 50 miles could provide data equivalent to that obtained on a recent survey of the west margin of the Atlantic Ocean along the U.S. coastline. It is proposed that data be obtained by continuous measurement of seismic, magnetic, and gravity effects on one ship and that bottom sampling and coring be carried out intermittently from a second vessel.

Other sections of this report contain proposals for investigations in the little-known regions of the South Pacific. In the course of such investigations, it may be possible to acquire additional information on the distribution, composition, and abundance of manganese nodules and phosphorite deposits.

Along the midocean ridges and the "rift" zone, the Mid-Atlantic Ridge is a project area sufficiently removed from individual countries to be a good location for international cooperation and yet close enough to be convenient to the American and European countries likely to provide the majority of personnel, ships, and equipment. Activities on the ridge might include surveys for hydrothermal deposits similar to those discovered in the Red Sea.

One and one half times the oil found to date in the world must be discovered and developed during the next 30 years. A major fraction will be found offshore. The oil industry is already exploring the continental shelves, drilling in deep water and developing offshore production and storage facilities. This work, essential to world commerce and industry, will be stimulated and supported during the IDOE by the proposed broad-scale reconnaissance geophysical surveys and deep coring (thousands of feet) to yield regional sediment description. Moreover, all projects directed toward characterization of the ocean and shallow ocean floor and toward environmental prediction can aid in the recovery of offshore petroleum. Determination of wave forces and of chemical and biological processes acting on structures is required for their economic installation and maintenance.

AN INTERNATIONAL COOPERATIVE STUDY OF EASTERN ATLANTIC CONTINENTAL MARGINS

As we have shown, most of the mineral resources of potential importance in the near future are expected to come from the shallow continental shelves and the adjacent relatively shallow sea floor of the continental margins. Thus, important emphasis should be given to investigations of such regions during the Decade. Individual countries should be encouraged to conduct and to publish surveys of their own continental shelves and slopes and to cooperate in regional studies of these features.

In this section we propose a cooperative study of the continental margins of the Eastern Atlantic as a program of the IDOE. This region has been selected because it borders so many nations, the continental margins are little known, they can be compared with relatively well-known margins of the Western Atlantic, and because the margins of the Atlantic can tell us much about continental drift and other fundamental scientific problems.

Few shelves are considered well known from the geological point of view. Although excellent scientific work is done in European countries, the small size of individual countries and of the parts of the continental shelf over which they have jurisdiction has tended to lead to detailed local studies rather than broad regional ones. As a result, regional aspects of marine sediments must be estimated from general knowledge of ocean currents and the expected effects of temperature and salinity of the water, combined with a knowledge of the contribution of sediments by streams that drain adjacent land areas. Check points are provided by a few regional and many local studies of the sediments on the shelves. Because of the uneven distribution of sample studies, we have only recently recognized that most of the area of the world's shelves is covered by relict sediments. Most of these sediments were deposited during times of glacially lowered sea level.

The structure of continental shelves is less well known than their sediment cover; information on the former depends mainly upon surveys using geophysical techniques. Such surveys clearly show the seaward prograding of continents, the past alternation of erosion and deposition, and the presence of buried folds, faults, unconformities, salt domes, and calcareous reefs—all important potential traps for oil and gas. Altogether only about 100 seismic-reflection profiles that cross the shelves of the United States and about 50 that cross the shelves of the rest of the world have been published (more have been made by industry but are not generally available). These profiles, supplemented by other geophysical data, rock dredgings and projections of structures known on land, permit a general classification of the structures underlying the continental shelves of the world.

Regional studies of shelf geology by oceanographic institutes, universities, or government organizations are of great interest to oil and mining companies. These detailed studies are necessary because of the large investment required before exploitation can begin. Really large regional studies of the continental shelf are most appropriate for international cooperation.

A general reconnaissance of the eastern Atlantic continental margin should solve many questions about sediment patterns on opposite sides of oceans, continental progradation in different climatic and physiographic zones, and the possible ancient breakaway of North America and South America from Europe and Africa. The reconnaissance should also reveal many problems in geology and biology that can be investigated later by scientists and engineers of the developed countries. The survey may also locate mineral resources that are unsuspected.

Such a regional study of continental shelves has its precedent in one that is now being conducted along the Atlantic Coast of the United States and extending into Canada and Mexico, as a joint investigation of the Woods Hole Oceanographic Institution, the U.S. Geological Survey, and the U.S. Bureau of Commercial Fisheries. Another region that includes much of the continental shelf off eastern Asia is being studied by national organizations of Asiatic countries under the auspices of the

United Nations Economic Commission for Asia and the Far East with the help of American and European scientists.

Discussions have already been held with European scientists concerning the proposed Eastern Atlantic study, and an international working group has been established by the Scientific Committee on Oceanic Research. The group is organizing a symposium to review present geological knowledge of the continental margins between Novaya Zemlya in the Arctic Ocean and Capetown, South Africa. The known gaps in knowledge suggest that agreement will be reached on the need for an international cooperative exploration of the region.

A ship will be required that is well equipped with instruments for measuring seismic reflection, geomagnetism, and gravity. It should also have adequate facilities for obtaining bottom materials during the geophysical traverses and much larger samples during special geological and biological sampling traverses. Suitable equipment and facilities are available on several U.S. research vessels.

Scientists from the United States may wish to participate in cooperation with those from European and West African countries of the Atlantic Coast. In order to diminish the self-limiting effects of national boundaries, it is suggested that individual scientists or groups of scientists undertake to investigate and report on a regional (not local) basis the results that are obtained in different units of the study. For example, one group should be responsible for the complete study of foraminifera from Novaya Zemlya to Capetown; another group, the sediment texture; another group, the seismic profiles; and so on.

SMALL OCEAN BASINS

Small ocean basins comprising mediterranean and marginal seas form a class of structures that appear to be intermediate in character between continents and ocean basins. The thickness of the crust (depth to Moho) is somewhat greater (12–14 km) than in most ocean basins but is decidedly less than for the continents in general. Small ocean basins lie close to the continents and receive large quantities of detritus from them. This material is known to form thick deposits of sediment in the few areas that have been studied. The sedimentary sections best known structurally contain relatively undisturbed beds that represent modern sedimentation to ages estimated at 1–2 million years. Apparently these are supplied by many small rivers whose deltas are not obvious features of the topography as well as by a few large ones whose deltas are prominent. These beds are underlain by layered rocks several kilometers thick that have been complexly folded and faulted. Little else is known of them.

Geological data from the Mediterranean Sea can be interpreted to suggest a change from continental conditions (i.e., probable high mountains) to deep basins in periods of the order of 10 million years, and many small basins elsewhere are seismically active, suggesting similar modification. Elucidation of the mantle interactions that presumably cause these changes should be an important scientific objective.

In common with the ocean basins there has been no economic exploitation of the deepest parts of these seas, but the borders of some, notably the Gulf of Mexico, have been extensively exploited for petroleum. Diapiric structures have been found in large numbers in the deepest parts of the Gulf of Mexico and the Balearic Basin of the Mediterranean Sea. Recent deep drilling has shown that the diapirs of the Sigsbee Deep in the Gulf of Mexico are salt domes and that the adjacent sediments bear oil.

Small ocean basins are highly effective traps for material spreading outward from the continents. This is true for undesired wastes and pollutants as well as for valuable minerals. Therefore, remarks about the importance of studies of chemical alteration and transport made elsewhere in this report apply with special force to these areas. Many of the areas are heavily populated, and we simply do not know their state of alteration or whether they are already partially polluted by man.

We recommend a geophysical and geological investigation of small ocean basins, including assessment of mineral resources potential, particularly petroleum; general technological information; and elucidation of tectonics and sediment dynamics.

The highest concentration of previous effort is in the Gulf of Mexico and the Caribbean, with the Mediterranean second. There are important residual scientific questions and many practical ones that might be answered in the Gulf of Mexico–Caribbean region, but the Mediterranean has received so little concentrated attention that it should be a prime target of early programs. The East Indies from the Philippines to India and Australia presents a considerable challenge and should be a prime area for cooperative studies.

The Red Sea is of special interest because of the hot brine occurrences discovered there on two or three expeditions of short duration. A more extensive program of investigations is amply justified. Other regions to be considered include the Norwegian, Bering, Labrador, and Chukchi Seas, the Sea of Japan, the Sea of Okhotsk and the Black Sea.

The most effective and economical tool for providing the preliminary survey is the seismic-reflection profiler since it provides an immediate indication of structure and thickness of sediments. It is the appropriate reconnaissance equipment and its use should be followed, where indicated, by examination of sediments and rock by means of exploratory drilling. This work should be heavily supplemented by seismic-refraction measurements and oblique-reflection profiles in order to measure thickness of the layered rock. These measurements should penetrate the full thickness of the earth's crust in order to provide a scientific dividend, possibly as important as the search for petroleum-bearing structures. Magnetic and gravity measurements are also essential for the thorough investigation of these areas.

DEEP DRILLING

The spectacular successes of the Deep Sea Drilling Program have proved the importance of drilling into the sediments that cover the ocean floor. These cores reveal not only the history of the ocean basins but also the existence of potentially valuable resources. The first deep hole drilled for this program suggested that the Gulf of Mexico may once have been a hypersaline basin largely isolated from the Atlantic. Salt deposited at this time has since risen as domes and anticlines piercing the overlying sediments. On the continents, turned-up edges of such pierced sediments form excellent petroleum traps.

Although the present drilling program will reveal many of the secrets locked in the sediments of the deep ocean basins, only very limited work will be done on the small ocean basins of the world and on the continental margins. Yet these are the regions where resources are most likely to be found. We suggest, therefore, that the Deep Sea Drilling Program be continued during the Decade with emphasis on basins such as the Gulf of Mexico, and the Mediterranean, Aleutian, and Black seas. The cost of each hole has been estimated at $300,000. One hundred carefully selected holes would provide the kind of reconnaissance information needed to gain an understanding of the development and resource potential of the sediments in these basins.

PLATE TECTONICS

Following the initial recognition of the plate-like behavior of each of the major units of the earth's surface, a great reorientation of thinking about geologic structure began. The plate concept is very recent, and much revision will be made during the Decade. In order to catalyze this change and to help the new concept order our interpretations of the sea floor, we suggest an integrated global study during the Decade. Since crustal plates move both apart (midocean ridges) and together (trenches and island arcs), we will first list the studies of these two very different phenomena, and then suggest programs that attempt to explore the underlying causes for the plate motion.

Although the motion away from the midocean spreading axes is well established from the magnetic anomalies, details of both volcanic and tectonic events at the axis of spreading are little known. Surveys based on echo sounding, surface magnetometers, and dredging have been carried down to the half-mile resolution of the techniques.

Deep-towed magnetometer and echo-sounder surveys of the Gorda Ridge were an early attempt to get finer detail. Observations on the extent of single basalt flow units, on the inferred fault surfaces, and on the exposures of greenstones and ultramafic rocks should clarify the processes that make the oceanic crust. Of primary interest is the question of whether the axial valley can be a steady-state feature of the terrain. Techniques of observation and sampling with navigation accuracies of tens of meters, and with a hard-rock sampling capability coordinated with visual observations are necessary. The appropriate mix of unmanned (remotely controlled) and manned instruments or devices (deep submersibles) for these tasks should be the subject of a cost–effectiveness analysis.

At depth beneath the midocean ridges, the problem of emplacement of the new upper mantle is unresolved. The only field observations likely to be of help are geophysical, but it is not yet clear which of several possible seismic or other techniques would be most effective. Exploratory geophysical surveys should be initiated to clarify the upper 50 kilometers beneath the ridge, with a more intensive (and expensive) observational program to be contingent on earlier successes.

Convergences in the plate-motion zones of trenches and young mountains have been studied less, although in terms of human interactions with volcanoes, earthquakes, and mineral resources, they may be far more important. For example, the continental shelf facing a deep trench is sometimes petroleum-rich, but the accumulations are a very special class of intensely normal-faulted oil fields. Moreover, part of the process of destroying oceanic crust may be the partial melting and sweating out of volatile and low-melting elements. It has been suggested that the chlorides that appear in volcanic gases and hot springs behind the trench are actually the oceanic chlorides from the marine sediments. If so, both the metals and the depositing solutions of the associated ore deposits are remobilized oceanic materials.

In addition to the resource potential of convergence zones, several major scientific problems have been identified:

1. There is a mechanical paradox concerning the undeformed sediments in the trench floors. If the sea floor is being jammed into the continent, why are the sediments not being wrinkled into folds? Although it appears that gravity sliding on the continental slope may have obscured the folding zone from acoustic view, a study of the zone of emergence of the earthquake belt is needed.
2. All previous calculations of the geochemical balance for the earth's surface have assumed that a certain amount of igneous rock has been transferred by weathering into so much sedimentary material. Now we must recognize the possibility that some of the unfusible material is returned from the sea floor to the mantle. The sodium balance has been particularly unsatisfactory. Now we can make a fresh start for each element; for the important isotopes, we can estimate the concentrations, and therefore the delivery rate, of marine crustal material. A tally of the volcanic delivery rate plus the hot springs flux would allow one to calculate, by difference, the composition of the material being returned to the mantle.
3. The latitudinal zonation of some marine trace elements in marine sediments should show up in the equivalent volcanic rocks behind the trench. Specifically, the high barium concentration in the equatorial marine sediment zone should be sought in an equatorial volcanic belt. Likewise, the coastal igneous rocks would be more calcium-rich if they were emplaced after the Cretaceous rise of the pelagic foraminifera.

In view of these questions, we recommend a study, to be called MANCON (Mantle Convergence), of the single clearest example of a convergence zone to be found. The primary criterion is a maximum length of legible record. A low sedimentation rate at sea would allow one to dredge and core old structures without having to drill through a great thickness of very young sediment. On land, mountainous terrain in an extremely arid climate is most likely to meet the criterion. Testing of the proposed latitudinal chemical zones requires that the example extend from the equator to 10 to 15 degrees north or south.

There are only three trenches that cross the equator: the Marianas Arc, the Java-Sumatra Arc, and the Peru-Chile Trench. Of these, the Peru-Chile Trench has behind it the magnificent relief and the extreme aridity of the Andes. Not only are the volcanic rocks of the last several million years available for study in the Andes, but the major canyons cut completely through the volcanic pile to a prevolcanic basement.

At sea, because of the lack of coastal rivers, the trench is not filled with sediments, and dredging is possible on the continental slope. From a resource point of view, there are sizable natural oil seeps on the continental shelf in a region as yet little explored for petroleum.

We suggest that two or three cross sections across the entire structural belt of the Peru-Chile Trench and its associated volcanoes be studied in great detail. These sections would cross trench, slope, shelf, volcanic belt, the mining belt, and the uplifted but nonvolcanic highlands. The geological program of dredging and coring at sea and of sampling on land would be aimed at the major geochemical balance. Geophysical profiles (gravity, magnetics, heat flow, reflection profiling, and deep refraction measurements) would be made along the cross sections at sea, and a carefully intercompared set of geophysical measurements would be continued on land. In addition, detailed earthquake seismology studies of submarine earthquakes (using land-based seismometers) would give primary information about present deformation and energy release.

The marine program would require about half a ship-year, in several one- or two-month cruises. Approximately six scientists and three technicians would be involved in the fieldwork and subsequent analysis throughout most of the Decade. The program offers an opportunity for collaboration among scientists of the United States and Peru.

As a complement to this continental-margin study, studies of a purely oceanic trench or trenches would be useful. The on-going program of study of the Tonga-Kermadec trench, the Solomon Island-New Guinea crustal convergence zone, and the proposed US-USSR study of the Aleutian Arc should be identified as important contributions to the Decade. During the Decade, a survey should also begin unraveling the complexities of the southwest Pacific complex of arcs from Indonesia to the Marianas.

Although we suspect that the plates move in response to the vector sum of several forces on them, the nature of the forces is not at all clear. If there are convective motions in the upper mantle, we are not yet able to identify the convective pattern. Satellite gravimetry cuts off at wavelengths larger than many of the features of interest. A better picture of the global gravimetry at higher resolution can be obtained during the Decade by setting up a coordinated program to place gravimeters on ships that are sailing useful tracks and to compile past and incoming results.

Seismic travel times from distant earthquakes are also useful well away from the oceanic ridges, both to delineate P-wave travel-time anomalies and to study shear-wave attenuation. Where anchored monitoring instruments are being emplaced during the Decade, recording bottom seismometers could be attached.

In light of the great importance to science of the 1954 Pioneer magnetic survey in the northeast Pacific, programs for extending the magnetic coverage, such as that of the Naval Oceanographic Office, must be encouraged. Consideration should be given to use of an aircraft carrier and propeller-driven aircraft to fly magnetometers. A set of aircraft tracks at five-mile spacing perpendicular to the ship's track would furnish rapid coverage. Flanking escort ships out near the limits of the flight paths would serve both for navigational monitoring of the aircraft tracks and for echo sounding. A single aircraft carrier of moderate size could complete the detailed magnetic survey of the world ocean in the first two years of the Decade and thereby provide the base charts to guide subsequent studies.

DEEP OCEAN EXPLORATION

While the resources of the various continental and island shelves are the most amenable to exploitation at present, and will be for some time, they are also those with the greatest potential for international controversy. The resources of the deep ocean, however, present a different problem. Because the economic potential of the deep ocean is largely unknown, and because this region is now free of the problems of claims of national jurisdiction, it is of great interest to all states, particularly to those that are not economically developed.

But the economic payoff from deep ocean exploration is far from definable. Nevertheless, the deep ocean cannot be neglected merely because its resources cannot be recovered immediately. A substantial part of an international program of ocean exploration should be devoted to a systematic description of the distribution of various properties of the deep ocean. This would involve a carefully designed survey of constants and variables not biased toward the solution or explanation of any specific problems but guided by existing knowledge. It would not be directed toward any specific resources but would measure the same general parameters discussed in continental-shelf projects, but on a much grosser spatial basis. Once the general appearance of the structures and properties is known, as it is already in significant areas of the ocean, the more intensive research or resources exploratory work can be done more efficiently.

The ships required for deep ocean work should have more range and endurance than those used in the shallow coastal areas, so they will be more expensive. This suggests that a sharing of these facilities and associated talent might be possible, and that a truly multilateral cooperative effort could be organized. The objectives would be the production of descriptive products such as those described by the National Academy of Sciences Committee on Oceanography in its suggestions for ocean-wide surveys, or those produced by the Environmental Science Services Administration Coast and Geodetic Survey in its SEAMAP program. It is conceivable that prearranged deep-sea tracklines, with relatively wide spacing, could be run to and from various coastal project areas by the vessels involved. This would require coordination in the planning phase, some standardization of methods and reporting, and the international distribution of the resulting data.

It should be noted that the transitions from deep ocean to shallow water is the locus of problems related to definitions of coastal state jurisdiction over the continental shelf. To the extent that limits are based on bathymetry, it will be necessary to survey and chart this transition region in considerable detail. Many countries lack the technical capability to do this work. An important part of the Decade should be the conducting by coastal states of the necessary surveys and cartography on a cooperating basis, including the provision of appropriate technical assistance where required.

THE VITAL STUFF
Biology and Living Resources

INTRODUCTION

For several millenia, man has obtained some part of his food supply from the oceans; this has always been an important, protein-rich component in the diet of peoples who have had access to it. Seafood remains an increasingly important source of protein in the hungry parts of the world; it is as well an increasingly important high-value, high-flavor component in the food economies of the less-hungry parts of the world.

In 1967, the living resources of the ocean yielded a harvest of more than 50 million tons, with a dockside value of about $8 billion. The basic exploration of these resources was not by means of deliberately planned resources surveys. Rather, over the centuries, fishermen have ventured farther and farther from home, and well before the earliest organized scientific survey they had discovered most of the major stocks of fish and invertebrates that we now use. Thus it was fishermen, not scientists, who discovered the resources of Antarctic whales, of Grand Banks codfish, of Indian oil-sardines, of Peruvian anchovies, and of many other species.

More recently, deliberate and systematic resource surveys by national and international agencies and by some of the larger units of the fishing industry have extended and made quantitative our knowledge of the stocks we are now exploiting and have identified, at least partially, the stocks we may exploit in the future.

These efforts continue to expand in this country and abroad in a manner that is difficult to assess; it must not be thought that the International Decade of Ocean Exploration (IDOE) will or should supplant these efforts or that it will or should operate independently of them. The Food and Agriculture Organization (FAO) of the United Nations is presently making the first over-all global assessment of living marine resources and of their potential yields as part of the Indicative World Plan for Agriculture and Fisheries, and these estimates will be available at the start of the Decade.

These studies and others indicate that the ocean is an excellent and abundant source of high-quality protein. It is also a substantial source of edible oils. Man requires about 15 g of animal protein, or its equivalent, per day, or 5.5 kg per year, an amount that can be obtained from about 37 kg (81 lb) of raw fish or other marine animals. Thus, animal protein for the present world population (3.5 billion persons) could be obtained from 128 million tons of fish (2.5 times the 1966 production) and for the population of 6 billion expected in the year 2000, from 220 million tons of fish.

Currently, only 15 percent of the world supply of animal protein comes from the ocean. The above calculations are based on the unrealistic assumption of even distribution of seafood throughout the world and serve only to show the potential of the ocean as a source of animal protein. Increases in production and more widespread distribution are required if this potential is to be realized.

The ocean is also a substantial source of edible oils. These oils are produced in substantial volume as a byproduct of the whales and sardine-like fishes. The shortage of edible oils on a worldwide basis is well known to professional nutritionists; the problem is particularly acute in Southeast Asia. The ocean is a major source of the edible oils used in western Europe, such as the hydrogenated oils for margarine that are taken from herring, anchovies, and whales.

Both from theoretical calculations based on food-chain dynamics and from surveys of the unused or under-used resources, some experts have concluded that a harvest of 200 million tons can be obtained from the kinds of organisms now used, with no radical developments such as fish-farming or exotic new kinds of gear. By harvesting some of the smaller organisms not yet utilized, such as Antarctic krill, "red crabs"

(*Pleuroncodes*), and lantern fish (myctophids), the harvest could be increased by another factor of four or five, at least. Such increases will occur only if the cost of achieving them is sufficiently low. Although, of course, we cannot expect to eliminate all waste, nor attain uniform distribution of marine protein to the people who need it, it is obvious that the sea is capable of providing a significant part of the animal protein needed by a hungry world. It is also noteworthy that the current annual world harvest of 20 million tons of herrings, sardines, and anchovies, most of which is used only indirectly for human consumption, is being landed at an average price of $33 per ton, and thus is a very inexpensive source of animal protein.

Further exploration of the living resources of the ocean would seem to be a natural component of the proposed Decade; plans for this must certainly be considered in formulating Decade programs, and some possibilities for this are suggested in this report.

We wish to emphasize, however, that living resources are self-renewing and that their rational use involves principles quite different from the once-for-all nature of mineral-resource use. All species of marine animals, including those potentially edible by man, form in nature parts of dynamic ecosystems that may show fluctuations, but where, in the long run, a natural balance is maintained between reproduction and predation and between food supply and food requirements.

With the start of a new fishery, this system is disturbed from its natural equilibrium by the addition of a new and efficient predator, man, who may rapidly come to dominate the system and may seriously disrupt it, at least temporarily.

Simply to explore the location and nature of unexploited resources is therefore a less satisfactory reaction to the need for increased use of marine resources in the biological field than it may be in the mineral field. What is also needed is the information essential for the rational management and conservation of living resources. Such information is barely adequate for some exploited resources, inadequate for others, and entirely wanting for most; its collection is seen as an important part of the Decade, the principal goal of which is the increased use of the resources of the ocean by man.

The living resources of the oceans differ from the mineral resources in another important way: they are subject to great natural fluctuations in relative abundance and in location. The more clearly the reasons for these fluctuations are understood, the more accurately they can be forecast by fishery oceanographers and biologists; the better the forecast of abundance and location and the farther ahead this forecast can be made, the more efficient, and therefore less costly, can the catching of the resources become. Forecast, or prediction, of living resources has come to be recognized as an essential element in rational management and in economic use of living resources, and it seems natural to include in the proposed Decade activities that will enable us to make better predictions in the fisheries field.

Predation is not the only biological role played by man in relation to the ocean. Like other organisms, he discharges his metabolic products into the environment. In man's case, these consist not only of sewage, but also of chemical wastes, heat, radioactive materials, dredging spoil, and other solids. Inadvertently, pesticides from agriculture and oily substances from shipping and oil-producing activities are also introduced into the ocean. The deleterious effects of these discharges include harm to living resources; hazards to human health; hindrance to maritime activities, including fishing; and reduction of amenities. Not all effects are necessarily harmful, although most of them probably are. For example, it is conceivable, and at least demonstrable, that heat and sewage could be discharged in a manner to enhance primary production and hence the general fertility of the sea.

For the purposes of understanding and controlling the biological consequences of pollution of the marine environment, the primary investigations required are ecological in nature and are similar to those necessary to an understanding of the impact of man's activities as a predator on the living resources of the sea; many of the sorts of investigations suggested by this report will serve both purposes.

This survey of the basic problems of planning an International Decade of Ocean Exploration has led to two conclusions: that any plans for intensified ocean exploration must be made with full recognition of other plans already made, and that exploration alone will not suffice to increase, in a sustained manner, man's ability to extract food from the sea.

For the sake of convenience, the discussion that follows deals separately with fishery studies and with other biological studies. In both cases, a prime problem is to gain a better understanding of the relationships of one part of the web of life in the oceans with its other parts and with the nonliving environment. In the first instance, this knowledge is needed so that the effect on a particular population being harvested can be predicted or controlled. In the second instance, it is required for formulation and testing of theory on the flow of materials and energy through the food web. The required investigations for fishery and biological purposes are often indistinguishable, although their motivation may differ.

Findings under one set of ocean conditions do not necessarily apply to another. Therefore, we have recommended the examination of a number of important and representative regions, both near the United States and elsewhere in the world ocean. These regions include well-studied and over-exploited zones, places where a little additional information could result in a significantly increased food supply and areas where essentially nothing is known. They should be studied as complete systems, from the input of energy at the sea surface to the harvesting of large and complex animals. The basic problems will not be completely solved in this decade, but we must lay the groundwork for understanding and exploitation in the future.

FISHERIES STUDIES

Almost everything that lives in the ocean can probably be eaten or used by man in some form or another, through the development of foreseeable technology. Economically, however, many things that are theoretically possible may not be actually practicable. For this reason, estimates of the total potential yield of food from the sea have varied by more than an order of magnitude, depending on individual definitions of "potential." However, as already noted, there is reasonable agreement that the sustainable yield of things that we presently know how to use or that we can profitably use in the next few decades, is probably around 200 million tons, or about four times our present harvest.

It seems reasonable to confine the IDOE mainly to studies of these sorts of resources, rather than to place emphasis upon resources that are unlikely to be profitably harvestable until late in this century. We probably already know, or shall find out incidentally, enough about the latter to identify developments in our technology that might render the harvesting of such resources economically practicable toward the end of the century.

From an idealized point of view, fishery studies in a region may be considered to pass through a series of stages. From the initial, subjective *exploratory* stage, which may be performed either by fishermen or by survey vessels of fisheries agencies, studies pass to the *descriptive* stage, in which quantitative stock assessments are made and life histories of constituent species are determined. Next, in the *dynamic* stage, the parameters of population response to the environment and to fishing are determined, and finally, in the *manipulative* stage, the knowledge acquired previously is used for the management of the resources and for prediction and control of future yields.

In practice, modern fisheries tend to develop in a nonlinear fashion, so that this series of stages is chiefly useful as an outline of the kinds of studies required.

Of the existing studies, including those being conducted within seas of importance to the U.S. domestic fishery and those in the oceans generally, some have proceeded only through the first stage; others are more advanced. In this proposal we have identified areas in which extra effort of a cooperative nature should be directed in order to bring studies in at least most of the oceans close to completion of the second, or descriptive, stage.

If the IDOE is to have a major effect upon U.S. domestic fisheries, it must encourage the completion of studies through all stages necessary for efficient and rational extraction of resources on a sustained basis, including the solution of relevant institutional problems.

A rationale for the research needed to enhance man's utilization of both U.S. and world fisheries, for the benefit of all, is suggested below, with a possible program that uses the Arabian Sea as a model. Further elaboration of this example into an operational program and the development of similar programs for other areas will require the attention of appropriate specialists.

A National Goal

Although the United States is an important consumer of fishery products, and although national consumption of these is steadily rising, production from the domestic fishing industry has remained relatively constant. Presently, about 70–75 percent of fishery products used domestically are imported from abroad, some being produced by United States overseas fishery enterprises.

Even conservative estimates suggest that much of the deficit between domestic production and consumption could be made good from underused resources known to be present off the coasts of the United States. Some experts have proposed as a national goal that domestic fishery production be doubled by 1980 and doubled again by the end of the century. Others doubt that such goals are economically realistic. Programs of the Decade should contribute to the resolution of this question.

For production to be increased to the extent proposed, or even significantly over present levels, a number of problems must be solved; some of these are the subject of proposed programs of the IDOE, while others, no less important, do not have a technical solution.

Some problems of the latter type could frustrate the goals of the Decade if they are not solved. Specifically, continuation of present institutional constraints to rational management could permit our resources to deteriorate below the level at which it is economic to harvest them, or it could prevent the utilization of virgin resources already known, uncovered, or assessed during the IDOE.

To indicate how the IDOE might assist the U.S. fishing industry to achieve a new level of production, it is proposed that the portions accessible to domestic fishermen of the following four regions be investigated: the Gulf of Mexico and Caribbean Sea; the Gulf of Alaska and adjacent parts of the northeast Pacific Ocean; the continental shelf and offshore banks of the northwest Atlantic Ocean; and the Eastern and Central Tropical Pacific Ocean.

Gulf of Mexico and Caribbean

The region extending from South America to Cape Hatteras, including the Caribbean Sea and the Gulf of Mexico, consists of interrelated ecological systems connected by circulation that originates in the equatorial current system, passes through the Caribbean and Gulf of Mexico, with some flow outside the Antillean chain, and leaves the region as the Florida Current or Gulf Stream. In part, the region is continental, in part insular. Two massive drainage systems influence the area: the Amazon and Orinoco Rivers of northeast South America, and the Mississippi River in the Gulf of Mexico.

The ecosystems of this region support a growing production of many high-value species and contain large stocks of valuable under-used species or unused species of apparent commercial potential. Although by world standards studies of ecological processes are advanced, they are still inadequate for fishery prediction and management in almost every area of research. A number of national and international cooperative investigations are being developed in the region, including the Gulf Estuarine Inventory, the proposed Gulf Science Year (in 1971), fishery development surveys in Colombia, Mexico, Venezuela, and the Central American countries [all under the United Nations Development Program (UNDP) and FAO], and the Cooperative Investigations of the Caribbean and Adjacent Regions (sponsored by the Intergovernmental Oceanographic Commission and scheduled for the early 1970's). Integration and coordination of these and other proposed programs within the IDOE should permit a significant increase in our knowledge of this region.

For the U.S. domestic fisheries, the Gulf and Caribbean region is a growing source of high-value food items (shrimp, snapper, tuna) and a growing source of relatively low-value fish (presently used primarily for fish meal and pet food) produced both by specialized fisheries and as byproducts of other fisheries. There are strong indications that it supports large unexploited resources of both kinds of products, and it is essential to bring studies of this area at least to the beginning of the dynamic stage during the course of the IDOE.

Gulf of Alaska

The Gulf of Alaska lies within the subarctic Pacific Ocean. Its circulation is dominated to below 200 m depth by the counterclockwise Alaska Gyre. Surface waters have relatively less salinity and are underlain by a well-defined halocline. On the eastern side, the shelf is fairly narrow and the slope is steep, while in the north the shelf widens as it curves to the west and southwest off the Alaskan Peninsula. From Unimak Island west along the Aleutians, the shelf is extremely narrow and the slope very steep. The shelf oceanography is poorly known, and not a great deal is known about the benthic infauna, particularly in the major part of the Gulf of Alaska.

Extensive trawl surveys during the past several decades have provided a relatively good understanding of the demersal fish populations of the region and have defined some of the more important invertebrate populations. During recent years, substantial exploitation by fishermen of several countries has led to maturation of the demersal fisheries in the Northeastern Pacific, and it is believed that for some species, such as yellow fin flounder and Pacific Ocean perch, over-fishing may already be taking place. This may also be true for some of the invertebrate populations, particularly king crab, which has been subject to intense fishing during the past decade and for which total catches are now declining rapidly.

One of the greatest unknowns for this area is the magnitude and composition of the stocks of pelagic fish other than salmon and herring. For semidemersal species not fully available to trawl gear, there are serious questions concerning the accuracy of the present estimates of stock abundance. Although detailed information is lacking, there is evidence of substantial stocks of scallops, pandalid shrimp, and Tanner crabs that could support expanded and diversified fisheries.

During the period of Ocean Decade, the following programs should be given high priority for the Gulf of Alaska region: (1) develop timely methods for rapid stock assessment of demersal and pelagic (both oceanic and shelf forms) fish resources, using combinations of acoustical surveys or improved sampling techniques, or both; (2) develop methods of acquiring and analyzing data on exploited fisheries that will allow for more effective and timely management decisions; (3) complete investigation of demersal and pelagic invertebrate stocks of the Alaska Gulf and the Bering Sea; (4) investigate infauna populations (molluscs) throughout the Gulf of Alaska and the Bering Sea (including studies of factors inhibiting their use, i.e., toxicity of intertidal clams); (5) initiate investigations of the pelagic fish potentials of the region; (6) undertake investigations of shelf oceanography with the goal of understanding environmental changes and their influence on fish and shellfish populations of the area; and (7) determine general productivity of latent fish and shellfish stocks.

Northwest Atlantic

On the series of shallow banks from Georges to the Grand Banks and along the continental shelf of the United States is a rich fishing area, once principally harvested by the U.S. domestic fishery, but now shared by many fishing fleets, mostly from Europe.

The general nature of the oceanographic regime of the region is well understood, as are the general nature, the distribution, and the seasonal migration of the demersal fish stocks on the banks. The fish taken from the Northwest Atlantic, one of the major fishing areas of the world, are mostly of relatively valuable species and represent a significant part of the world's landings. Research in the area has been coordinated for a number of years through the International Commission for the Northwest Atlantic Fisheries (ICNAF). The descriptive phase of research on demersal fish stocks is nearing completion, and research on them is active in the dynamic stage. For example, ICNAF has under consideration a proposal for an environmental survey of the Georges Bank area in relation to the recruitment of commercial fish stocks there. It is expected that IDOE operations in this region would go far toward ensuring as great a sustained yield from this important fishery as becomes economically desirable, and they could also serve as a useful prototype for studies elsewhere. However, work directed at exploration of the pelagic resources of this area as well must be pursued during the Decade.

Eastern and Central Tropical Pacific

One of the major fisheries of this region, high-seas tropical tuna, in which the United States presently has the largest share, is facing the need for new adjustments of the operation. Yellowfin tuna has traditionally formed the bulk of the landings from this fishery, but yellowfin are now under regulation by an international commission, and the annual quota is taken by the fishery in approximately 6 months each year.

Additionally, new and more powerful vessels from other nations are entering this profitable fishery at an increasing rate, so the share of the international quota going to each fleet must be expected to diminish progressively with time.

It is, therefore, most urgent to seek new resources on which the tuna fleet may work; a certain measure of relief may be afforded by the stocks of yellowfin in the tropical Atlantic, but it seems likely that the unpredictable and underutilized stocks of skipjack tuna in the eastern and central Pacific and in the Atlantic must also be increasingly exploited.

While the fishing boats may be counted upon to investigate the skipjack stocks on their own accord to a certain extent, the distribution and migration of these fish over great expanses of the tropical Pacific suggests that there is a need for deliberate resource assessment, even during the scarcely begun exploratory stage of research on this species. This has already been initiated, in a small way, by the international EASTROPAC investigations, but it would seem to be pertinent to the intentions of the IDOE that increased U.S. and cooperative international effort be put into the search for an understanding of the magnitude of the alternative tuna resources in the eastern and central tropical Pacific.

In this region there are also populations of other exploited or exploitable living resources, such as anchovies, thread-herring, shrimp, and red crab (*Pleuroncodes*). The studies of physical, chemical, and biological oceanography that were essential components of the EASTROPAC investigation, and their further elaboration during the Decade, will be of great value toward total utilization of the other resources.

An International Goal

In the most general sense, a principal objective of the Decade is to increase the use of living resources in the world ocean and to do so on a sustainable basis. This goal pertains not only to the whole community of nations; it is also of direct national interest. Because of its considerable dependence on fishery products from distant waters, the United States is concerned with fishery questions over broad sectors of the world ocean. Of importance to this country are not only the development of new resources but also both the conservation and rational use of populations of animals providing food to the United States, and the decrease of cost per unit of production of those animals.

In view of the Indicative World Plan (IWP) study of FAO, it seems proper for the Decade to concentrate on a close examination of several selected regions in which particular attention should be given to resources identified by IWP as "hypothetical," being based only on an extrapolation from known resources and environmental conditions. In each of the selected areas, it should be the goal of the Decade to identify the nature and location of the latent resources, to examine the degree to which known resources are underutilized, and to make improved estimates of the probable harvest. The nature of the products to be extracted from the resource should be examined on a seasonal basis, and the relevance of these products to the local economy should be considered. Information from these investigations should be made immediately available to those concerned with investment and development in these regions.

In accomplishing these tasks, it is essential that there be an increased and related effort in basic systematics and in essential aspects of the life history of the exploited organisms and their relationships with other organisms in order to provide the foundations of information on which to base rational schemes of management.

As examples, a number of selected regions in which such studies could be carried out are proposed below. As noted elsewhere in this report, some of these areas have been identified for special attention for other reasons. Although research at some level will presumably be undertaken in each of these regions even in the absence of Decade stimulation, it should be a consequence of the Decade that such investigations are more strongly supported and coordinated. These would be cooperative investigations in which scientists from this country could participate with those of other interested countries.

The regions proposed, and the reasons for their selection, follow:

Arabian Sea

This is a rich and little-explored region, close to a large and poorly fed population. At present, fishing is limited or primitive. As a result of Decade investigations, it should be possible to attract private and public capital into the fishery, making it economically viable.

Antarctic

The region contains large unexploited and relatively inaccessible resources. In recent years, much work has been done by Soviet, American, and other scientists; this work should be reviewed and interpreted as a prelude to Decade investigations. Of particular interest are the large concentrations of krill and the notothenid fish resources.

Southern Chile

The area is potentially very productive but is now little exploited. Although offshore waters are rough and suitable only for distant-water fleets, much fishing could be done in semiprotected waters by local vessels, and IDOE activity in this region could perhaps stimulate increased usage.

Indonesian Shelf

This is a relatively unexploited area with potentially rich resources especially of bottom fish and shrimp, adjacent to a large population that could well utilize additional sources of animal protein and foreign exchange.

As an example of the sort of program we suggest might be developed in these areas during the Decade, and to illustrate the wide range and diversity of activities that might be appropriate, a more detailed and illustrative statement on the Arabian Sea follows.

The part of the Indian Ocean known as the Arabian Sea lies north of the equator and west of Ceylon, comprising the region bounded by India, West Pakistan, Arabia, and the Somali coast. As a result of exploratory work of the International Indian Ocean Expedition it has been established that portions of this region are among the richest parts of the oceans. Estimates of the maximum sustainable yield of fishery products of the sort now used in world commerce for direct human consumption and for animal nutrition are of the order of 10 million tons per year (about 20 percent of the 1966 world catch). Present production is now only about 1 million tons per year.

Our present knowledge of the region suggests many problems related to the development of the fisheries of the region, and it might be possible to make significant advances toward their solution during the course of IDOE investigations. Some of these are listed below in an approximate order of priority.

1. What are the causes of the temporal fluctuation in availability to the fisheries of the oil sardine (*Sardinella longiceps*) and the Indian mackerel (*Rastrelliger kanagurta*), on seasonal, yearly, and longer-term cycles; how can these fluctuations be predicted or stabilized?
2. What are the mechanisms responsible for the high rates of primary production and what governs year-to-year differences in the upwelling cycle in various sectors of the Arabian Sea? This problem is part of, and possibly inseparable from (1), above.
3. Why is there such a high yield of *Penaeid* shrimp off the arid Iranian coast? Many, or most, species of similar shrimp require a brackish habitat in their early life history, and comparison of shrimp life history off the coast of Iran with that off the Kerala and Mysore coasts of India, where brackish-water environments are abundant, should be of great practical interest.
4. Is the seasonal shallowness of the thermocline in the Gulf of Oman

alone responsible for the large quantities of fish observed there, or are other factors involved?

5. How do the fish stocks on the western Indian continental shelf adjust to seasonal conditions of less-salty surface water during the monsoon and to development of nearly oxygen-free conditions below the thermocline on the shelf during seasonal upwelling?

6. What is the explanation of the irregular mass fish kills on the Malabar coast and in the open Arabian Sea, and what is the frequency of their occurrence? What species are affected, and what is the effect on fisheries?

Little is known about the life history and not enough is known about the systematics of most of the fish species in the region. For instance, there may be as many as 50 species of poorly known sardine-like fishes, of perhaps a dozen genera, endemic to the region. They probably comprise a major element of the usable biological resources, and the development of management procedures can scarcely be approached until further systematic studies are carried out. Other groups of the highly diverse fauna are as little understood as are the clupeids.

A reasonable goal might be to increase the yield from this region to 10 million tons per year. The yield could consist of 6 million tons of sardine-like and other small fishes suitable for fish meal and protein concentrate, solubles, and dried or minced frozen fish; 3 million tons of suitably preserved fish for direct local human consumption; and 0.5 million tons each of shellfish and of large frozen fish for export to industrialized countries. If it were possible to achieve production at these levels and at a reasonable price, the income of regional fishermen could be increased by $1 billion per year; foreign-exchange earnings could be increased by about $700 million and animal protein for local consumption by about 600,000 tons per year. To establish the practicability of the goal of increased yield, scientific investigations should be accompanied by a detailed analysis of costs, of the existing market, and of the possibility of expansion of markets, in terms of new products and new areas.

Achievement of this production goal within a decade is unlikely because of institutional, social, and economic barriers. If such a goal is to be achieved by the year 2000, certain steps beyond the purely technical scope of the IDOE are required, including the provision of funds for construction of harbors, roads, and drydock and repair facilities; the provision of normal investment guarantees to foreign sources of capital; provision of funding to local nations for fishery development; and undertaking of a regional-planning study for fishery development.

Although these steps differ in character from the scientific and engineering programs of the Decade, their study and implementation should proceed in parallel with Decade programs if the broad goal of increasing utilization of the ocean and its resources is to be achieved.

Desirable environmental and biological studies in this area are mentioned in other sections of this report. To the extent possible, these investigations should be conducted as a cooperative project of the nations of the region. In addition, it would be extremely useful to station one or more research vessels in the region, with an adequate complement of scientists and technicians. These would provide platforms for long-term, comprehensive programs of ocean and atmosphere research in the way that *Eltanin* has served in the Antarctic during recent years. Not only could such vessels be used for more sophisticated investigations than might be possible on local vessels, but they could serve a useful role in training local scientists and in developing capabilities for advanced research in the region.

Necessary taxonomic research should be initiated early in the Decade, and in large part could be conducted in national or international institutions in the region. An important step toward a more precise assessment of regional resources could be made by analyzing fish eggs and larvae already available in collections of the International Indian Ocean Expedition. This could be done at least in part at the Indian Ocean Biological Center in Cochin, India. Results of this analysis could be used in designing subsequent field investigations.

OTHER BIOLOGICAL STUDIES

There are many biological problems in the ocean that have to be solved; some of them may be linked with the goal of increasing the production of food. From this wide range of problems, it seems appropriate that during the Decade particular emphasis be given to those whose solution may contribute to an understanding of the community structure and dynamics of ecosystems. Such problems involve an analysis of the flow of energy and the cycling of matter through ecosystems, the efficiencies of conversion between various trophic levels, and a host of related matters. Any improvement of our present level of understanding of ecosystem dynamics would be a major advance toward prediction and ultimate control of biological events. This understanding is closely dependent on physical, chemical, and meteorological investigations of the mechanisms of environmental change. Such knowledge could eventually be applied to more effective systems of biological production, and it is essential if rational schemes of mariculture are to be put into practice. From the fishery point of view, this understanding is required for the evolution of constructive measures of conservation and management of living resources.

An important approach is the application of existing analytical models of plankton production to the available data from a few well-studied areas of the world ocean. Modification of these models and development of new approaches like simulation modeling should be undertaken so that the models can be continually improved on the basis of new data and can at the same time lead to improved observational programs. By using existing models and data it should be possible to spell out clearly for the best-known areas what information would be needed in these areas to predict at least the phytoplankton cycles from easily measured (and monitored) parameters. The theory developed with existing data is to be checked in the later part of the Decade by work at sea specifically designed for this purpose so that the models can be accepted or rejected for the particular areas. If this were accomplished in the best-known regions of the oceans, we would have available the means for quantitative comparison of the rates of the principal processes supporting food chains in major climatic domains of the oceans. Further, it may be possible to bypass the collecting phase of the traditional kind in most of the remaining oceans. Also, once the driving parameters of phytoplankton development are well understood, this approach would allow us to assess the precision obtainable during future monitoring of plankton when the quality of the input in the model is given or can be set. Relatively well-known areas where existing data could be used include the Eastern Tropical Pacific, the Sargasso Sea, the upwelling regions off Peru and off Washington-Oregon, and the Alaskan gyre.

Another approach is to conduct further field observations in several selected regions of the oceans to obtain the data and understanding required for eventual dynamic modeling. Criteria for the selection of these regions were that knowledge of their ecosystems could serve as a model for systems in analogous parts of the ocean, that they were known or suspected to contain important fishery resources, and that they should include some regions of interest to U.S. fisheries and some of interest to fisheries of other countries.

The regions proposed, with some of the reasons for their selection, are as follows:

1. *Low-latitude oceanic gyre* (*South Pacific*): There are indications that the steady state in plankton communities may be approached in such a region, which could serve as a model for enormous reaches of

low and mid latitudes elsewhere. The region is little known, it includes the interesting and potentially productive coral-reef ecosystems, and it is likely to receive attention from other branches of oceanography, from which supporting environmental data may be drawn.

2. *High-latitude oceanic gyre* (*Gulf of Alaska*): This region has a relatively well-understood oceanographic structure with a strong seasonal cycle. The seasonal fluctuations of the plankton communities should be compared with those in the case above. There are important fishery resources that are moderately productive and are only partially exploited. Both shelf and deep-sea conditions are present.

3. *Tropical enrichment* (*Eastern and Central Tropical Pacific*): Within this region the phenomena of oceanic upwelling, divergences, doming and ridging, and eddying constitute a unique laboratory for the study of surface enrichment in the open ocean. The region is only moderately well known, and the relation between its ecosystems and midocean fish production is of obvious importance.

4. *Coastal upwelling* (*Western Arabian Sea*): Despite the intermittent nature of surface enrichment due to the monsoonal reversal in the wind field, this region is extremely productive and is believed to contain abundant fishery resources that are largely unutilized. Although important as a model for low-latitude biological systems with intermittent upwelling cycles, the area is unlikely to be studied intensively unless it is given prominence by a project such as the Decade.

5. *Midlatitude shelf and adjacent banks* (*Georges and Grand Banks*): Because of the long history of investigation, the region is relatively well known and further work should enable reaching a satisfactory understanding of fishery-resource dynamics. The area contains important resources and is the locus of significant conflicts of interest among several fishing nations.

6. *Very high latitudes* (*Antarctic waters*): The extreme latitudes, the presence of very great potential resources, the interaction between coastal conditions and the Antarctic Circumpolar Current, and the analogy with high-northern-latitude systems combine to support selection of this region.

7. *Inland seas* (*Gulf of Mexico*): This region can serve as a model for the enrichment of low-latitude shelf areas by riverine organic and inorganic material. It is also of great actual and potential importance to domestic fisheries.

It should be noted that several of these regions have been selected for both biological and fishery reasons, and some are already the object of proposed cooperative studies, both national and international.

Primary attention in each case should be given to measuring those physical, chemical, and biological factors that are most critical for understanding the dynamics of the particular ecosystem. This will make possible the introduction of realistic values for the parameters of mathematical models of the ecosystem. Some particular problems that fall within the scope of the IDOE are included in the following illustrative examples:

1. What are the relative roles played, in a variety of near-shore ecosystems, by organic matter reaching the system from various sources: phytoplankton, benthic algae, and terrestrially produced riverborne material? The importance of terrestrial organic material to the demersal fish and benthic invertebrate ecosystem, especially in the tropics, should be explored.

2. What are the factors determining the complete utilization by plants of available inorganic nutrients and the apparent paradox of available but unutilized nutrient salts in some oceanic conditions? This problem may also include consideration of the factors, other than vertical advection of nutrients, that lead to the general phenomenon of relatively high production and complete nutrient utilization in proximity to coasts.

3. What role is played by the very small members of the food chains (a) in the energy cycle of planktonic communities and (b) in the utilization of organic material available to benthic communities? Such studies as have been completed indicate that this part of the size spectrum of community members is of prime importance, but the studies are still very few, and have been done in only a few scattered localities of the ocean.

4. How does organic material reach the communities of organisms in the ocean beneath the euphotic zone, and what are the relative roles of (a) active vertical migration by a series of species having vertically overlapping distributions, (b) of passive sedimentation from above, and (c) of horizontal advection from the origins of the deeper water masses? There appear to be some indications from the vertical distribution and carbon age of organic matter in mid-depths and from the findings of common, colorless flagellates below the euphotic zone, that some current theories are considerable oversimplifications. This problem is of importance in both planktonic and benthic ecosystems in the deep ocean.

5. What is the mechanism by which a high-biomass ecosystem, the oceanic coral reef, maintains itself in the tropical ocean, an apparently nutrient-poor medium? This problem concerns the sources of energy input into the reef ecosystems and the rates of the recycling of material within the systems.

6. What are relative roles played by biological factors and by the nature of the physical environment in determining the composition of the fauna naturally present in any region of the ocean? An understanding of this relationship in many ecosystems in the ocean could enable us to evaluate the possibility for increasing the productivity of a particular ecosystem by an artificial transfer of species from other parts of the ocean. Such replantings may include organisms at any trophic level within the system and may include both living resources of interest to man and the organisms used by these resources as food.

This list of possible topics for study is not comprehensive, nor is it intended to be; the spirit of this list, taken with the suggested concentration of effort in a number of selected regions, may form a framework on which individual projects can be developed. The IDOE, and especially its biological component, cannot be entirely under central direction, particularly because many of the participants will not be members of government agencies. At this stage we have defined only the general nature of biological studies that may be considered as part of the IDOE and have not attempted to enumerate all possible projects or even all those now considered to be desirable by the rather small number of scientists who have so far been consulted.

TWO FLUIDS
Physics and Environmental Prediction

INTRODUCTION

As society grows in the scope of its activity, as complex new industries take form, as demands for transportation increase, and as we transform the earth into a densely inhabited planet, we find that our need for a deeper understanding of the world around us increases correspondingly. It might have been sufficient for the solitary pioneer to know the ecology of the immediate surroundings of his homestead—the lore of the nearby forests and streams—but today our livelihood and prosperity are inextricably bound to that of peoples on other continents. The state of the rice crop in Indonesia, the monsoon rains over the farmlands of India, and years of bad fishing off Japan all affect us. Desperate, starving people make neither good customers nor good neighbors. Many of these fluctuations in livelihood and prosperity are linked to vagaries of weather and climate, which if anticipated and predicted, could be allowed for. Thus our need to measure, monitor, and understand the weather has grown to global proportions, and it is beginning to dawn upon us that on this global scale the atmosphere and ocean are as closely linked as two coats of paint on a croquet ball: they touch each other almost everywhere.

Meteorologists recognize the need to understand the machinery at work in this close bond between air and ocean and the need to gather data on a global scale. To reach this understanding, meteorologists have organized internationally through the World Meteorological Organization (WMO) a research program known as the Global Atmospheric Research Program (GARP) and a global observational network known as the World Weather Watch (WWW). Both of these programs recognize the necessity for deriving information from the ocean surface. It appears necessary to penetrate the ocean at least to the seasonal thermocline, since knowledge of heat storage in this layer will be required to extend the time range of weather forecasts. At the same time, the Intergovernmental Oceanographic Commission (IOC) is organizing a parallel observational network known as the Integrated Global Ocean Station System (IGOSS), which will monitor ocean conditions. It is clearly to the advantage of both oceanography and meteorology that there be close coordination of Decade programs with those already planned through WMO and IOC and that one of the main features controlling plans for the observational program in the Decade be that it is made of optimum usefulness to the needs of research meteorology.

A goal of the Decade should be to improve knowledge and understanding to the point that ocean forecasting for a variety of users can be conducted on a routine and effective basis, carried out in close collaboration with parallel investigations of the atmosphere.

In this chapter we begin by discussing the need for and prospects of a global numerical model for forecasting the joint ocean–atmosphere behavior. We conclude that during the Decade significant progress toward this goal can be made and suggest that preliminary studies of more limited scope are first desirable. Data now available in the Western Pacific should be used to devise and test simple numerical models. Concerted data-gathering programs should be initiated in certain limited regions, namely the Equatorial Pacific, the Somali Current, and a subtropical upwelling area.

In addition to these experimental regional studies, we should take advantage of gathering routine oceanic data on stations and ships established by the WWW. A limited increase in permanent ocean stations and island observations is justified, and it is suggested that several intensely instrumented island stations be established. New techniques, such as the free-fall transport indicator and the bottom-pressure gauge, require special field programs to assess their potential for global ocean-monitoring schemes. In the case of the deep ocean, even the mean state is ill-defined, and observations are needed in regions presently devoid of deep data, especially in inaccessible polar regions.

THE PECULIAR IMPORTANCE OF FORECASTING THE STATE OF THE OCEAN SURFACE LAYER

Oceanographers are well aware that the ocean surface layer is the part of the ocean of most immediate human concern. It is obviously the locus of most of man's ocean activities: surface shipping is influenced by surface weather and sea conditions; primary production is limited to the upper tens of meters, and most of the major fisheries operate in relatively shallow depths; coastal installations and offshore structures are subject to the vicissitudes of the sea surface; and most disposal of wastes and inadvertent contamination occur at or near the surface, which is also the domain of most recreational activities.

Until recently, the oceanographer has not been faced with the same demands for forecasting that the meteorologist encounters. Enhanced activity in the ocean and increasing understanding of oceanic processes have led to the development of relatively primitive forecasting systems. These are now being applied to fisheries, to the routing of ships, to national defense, and to the protection of coastal and offshore structures.

The procedure for formulating the forecast has to be based on a sound theoretical understanding of the processes at work in the upper ocean; with the exception of our knowledge of the generation of waves by the wind, this is progressing slowly. It is therefore appropriate to place major emphasis on studies of the surface layer of the ocean and its interaction with the lower atmosphere. The upper few hundred meters of the ocean are highly variable in heat content. Heat extracted from or added to this layer depends largely on characteristics of the overlying atmosphere—whether cloudy or clear (variable radiation input), cold or warm (variable exchange of sensible heat), moist or dry (variable evaporation), or windy or calm (variable exchange of momentum). But in releasing varying amounts of heat and water vapor to the atmosphere, the ocean often supplies the drive for storms, both tropical and extratropical, and may also influence the intensity of anticyclones.

Fluxes of heat, radiation, water vapor, and momentum across the ocean–atmosphere interface are of principal importance in studying the dynamics of coupling of both media. Ways of measuring these fluxes with the level of statistical significance that is necessary for a clear exposition of the mechanisms at work are being developed in other programs such as GARP. The practical art of forecasting will be advanced as the scientific understanding of the associated phenomena is developed, and this will constitute an important link between the IDOE and GARP.

Parallel with the development of the scientific base, means of adequate measurement must also be advanced. These involve difficult design problems, for the ocean is an untiring enemy of buoys, moorings, and other man-made devices. It will not be easy to build observation stations rugged enough to withstand winter storms, yet delicate enough to measure the relevant quantities. We anticipate that following the early stages of scientific measurement—and we hope, successful forecasting—we will require the eventual "instrumenting" of parts of the ocean, with networks of observation stations aided by earth-observing and communication satellites. Investigations of the IDOE should provide the basis for development of an adequate monitoring system and may include the installation of simple pilot array of buoys; construction, installation, and operation of the eventual global system would appear to be beyond the scope of the Decade.

However, one means of gathering oceanic data for monitoring purposes does exist—the ship of opportunity. It is already common practice in meteorology to use "selected" merchant ships from which routine measurements of surface weather elements are made at standard six-hour intervals and reported by radio to the global observational network. Such observations are used in "real time" by weather forecasters and are also incorporated in archives for climatological studies. In some cases, observers accompany the ships to make upper-air observations; during the WWW, it is planned to increase significantly the number of ships making such observations.

Data resulting from these observations are used by oceanographers, although the coverage, quality, and variety of observations leave much to be desired. Recently, trials have been conducted on a few ships from which expendable bathythermographs have been used, with the resulting data being radioed to the Navy Fleet Numerical Weather Facility in Monterey, California. For many years, Hardy plankton samplers have been routinely towed by certain merchant ships in the North Atlantic; since 1964, Coast Guard ocean station vessels have participated in this program. Other biological observations and collections have also been attempted.

During the last few years, a number of instruments have been developed for continuous recording of such properties as temperature, salinity, and chlorophyll *a* content. Equipment for automatic analysis of nutrient substances (such as phosphate, silicate, and nitrate) is now coming into common use.

The potential of ships of opportunity for obtaining additional oceanographic observations on a global basis should be carefully evaluated. Before an enlarged field program of this sort is initiated, it is necessary to identify the uses to which such data could be put and the organization that would take the responsibility for coordination and management, and to establish specifications for data, adequate communication links, and procedures for handling the flow of data. The following possibilities should be explored:

1. Where ships have been selected to carry trained observers, as planned for the WWW, the opportunity should be taken to incorporate oceanographic observations in the observational program. For example, the use of continuous recording of surface temperature, salinity, and chlorophyll *a*, and the use of expendable bathythermographs, should be considered.
2. An analysis should be made of merchant-ship tracks to identify a selected group of ships regularly covering tracks of particular oceanographic interest (for example, routine shipping across the equator between Hawaii and Tahiti). Equipment for measurements such as those proposed above could be provided; if special observers were not aboard, observations could be made or supervised by ship's officers.
3. Instrument packages should be developed for installation on a number of suitable merchant ships whereby a selection of useful measurements can be made automatically, with very little attention from ship's personnel. The most difficult problem may be to ensure proper navigational control.

WORKING TOWARD A GLOBAL FORECASTING MODEL

An oceanographic and meteorological problem of considerable scientific and applied importance, particularly to fisheries, is the question of large-scale year-to-year differences in the ocean. Interactions between ocean and atmosphere extending over periods of weeks or months appear to play a major role in the development of persistent oceanic regimes. For example, over time periods of weeks, months, or seasons, pools of anomalous warm or cold water are developed on a scale of 1,000 to 3,000 miles across. These dimensions are suggestive of branches of the large-scale "centers of action" of the atmosphere like the Pacific High or Aleutian Low. These pools can be associated with anomalies in the prevailing wind systems. It is desirable to document these phenomena more fully: to learn enough about their scale and frequency to determine the kind of measurement spacing in time and space that will be necessary to describe them. A beginning has been made in the study of features of this scale and frequency in the North Pacific buoy program, and during the Decade this work can be extended. Because of economic and other limitations, the results of this program should be reviewed as similar experiments are initiated elsewhere later in the Decade. The North Pacific was selected as a site of measurement because of its importance in predicting Northern Hemisphere weather and because of its interesting equatorial circulations and interactions.

It is necessary to determine, within the selected area, whether routine observations should be made from fixed or moving surface platforms, from aircraft, or from satellites. It is also desirable to decide, for example, what spacing of buoys and what frequency of sampling will be required in the design of a suitable data net. Such problems are being reviewed by the National Data Buoy Development Project. Some heavily sampled areas such as the West Pacific can possibly be used to assess the data needs for forecasting of ocean and air condition as a coupled system. Studies of new high-density blocks of data have to be encouraged for these purposes in the hope that they will lead to well-designed data nets during the IDOE.

In contemplating such large-scale nets, attention should be given to the establishment of simple shore stations in developing nations. Simple tide gauges and recording sea thermographs at coastal locations would yield useful information and would help increase interest in oceanography in such countries.

Cargo transfer through coastal ports is of great consequence to the world economy and is of special importance for the exploitation of natural resources and other aspects of the economic development of coastal states. Certain engineering-data requirements must be met if the most advantageous sites are to be selected and if channels and structures are to be properly designed. Among these factors are:

1. Wave climate and knowledge of storm wave action
2. Effect of tsunamis
3. Littoral drift and migration of inlets, bars, and channels
4. Supply of shoal material from inland and other sources
5. Tides and tidal currents
6. Flow characteristics of streams affecting port areas

The period during which measurement of these factors must be made will depend on the complexity of coastal processes and the present availability of information for a given area. Similar data are required for the development of offshore facilities for handling very large ships, such as the supertankers.

Although some phenomena described by these factors are of large dimensions, the required detailed studies have a very local character and must be made within territorial or internal waters of individual coastal states. Within the mutual-assistance aspect of the Decade, it would be appropriate for the United States to offer technical assistance on such programs through bilateral or regional arrangements.

Part of the data system for a large-scale forecasting system already exists. It consists of ESSA, NIMBUS, and ATS spacecraft, weather ships, conventional ships, and oceanographic ships. Other data-gathering systems are under active consideration. They are new sensors on aircraft, spacecraft, and buoy arrays to measure conditions at the air–sea interface and in the oceans. Also, satellite data-collection systems that could solve the data-gathering problem have been proposed. A cost–effectiveness study is needed of the entire complex of presently available and proposed systems for gathering oceanographic data, including aircraft and spacecraft system, anchored buoys, surface buoys, weather ships, conventional ships, and oceanographic ships, in order to determine which mix of systems will provide the most data at the lowest cost. Questions of whether every ship at sea should be provided with an anenometer at the expense of, say, "X" more oceanographic buoys have to be answered. Would the cost of having all ships at sea provide observations to a collecting spacecraft be cost-effective versus some other competitive system? It is hoped that the studies being conducted by the National Data Buoy Development Project will resolve these problems.

Because of the progress in meteorology over the past decade and recent advances in the development of numerical models of the oceans, we can foresee in general terms that a global model of the ocean is possible. It could be based on the "box" model developed at ESSA. This "box" model has a high degree of versatility in that the horizontal and vertical spacing of grid points need not be uniform. Therefore, the upper layers can be defined more diversely in the vertical, and areas of rapid variability in the horizontal, such as the Gulf Stream meanders and the shallow, narrow, swift equatorial undercurrent, can be adequately resolved. The complexity of such models is limited by present theoretical deficiencies and by computer speed, memory capacity, and input–output problems, but an ocean model with 10,000 or 20,000 data points for 20 to 40 depths in the ocean for which changes in half-hour time steps would be computed is feasible within the early part of the Decade.

Moreover, the mathematics of these models is basically simpler than the mathematics of earlier models. The newer models are based on the primitive equations of motion, and problems of false oscillations, computational instability, and aliasing have substantially been solved.

PRELIMINARY FORECAST EXPERIMENTS FOR THE DECADE

We suggest conducting forecast experiments in three different oceanic regimes: (a) the Somali Current, where an existing preliminary numerical model might be used in the design of the observing system; (b) the Western Pacific and the China Seas, where numerous available data could be used in constructing a preliminary numerical model; and (c) the Equatorial Pacific, where present data are probably insufficient for a preliminary numerical model. Results from the first two experiments would contribute to the design of the third. Consideration should also be given (d) to similar studies in a subtropical upwelling region.

Somali Current and Arabian Sea

During the International Indian Ocean Expedition, it was established that the pattern of flow in the Indian Ocean is much like that observed in subtropical gyres of other oceans, with the exceptions that it changes direction with the monsoons and it straddles the equator instead of one of the tropics. The Somali Current was found to resemble the Gulf Stream or Kuroshio in detailed structure, and although it is quite clear that it is generally synchronous with the regime of monsoons, the time of response is unknown. Estimates vary between a phase lag of current behind wind of two months to a phase lead of one month. This question of the dynamic response of the ocean to large-scale wind features such as the monsoon and response of the monsoon to the ocean characteristics is crucial for theory of ocean circulation as well as to the problem of predicting the onset, intensity, and termination of the monsoon. A detailed theoretical study of the Somali Current has recently been completed, and a numerical model has been developed, so there is a basis for an enlightening interplay of observation and experiment.

Some attention should be given to possible programs of measurement of the Somali Current by geomagnetic electrokinetograph (GEK) on a regular basis over a four-year period; the establishment of weather stations along the coasts of Somalia and Socotra should also be encouraged. If a few weather buoys could be placed in the monsoon area in the Arabian Sea, and some south of the equator as well, the data could be combined with those available from the Indian coast to give a much better picture of the time of onset, the structure (on mesoscales and synoptic scales), and the duration of the monsoons over the ocean as a whole. Perhaps radio tracking of very-low-level balloons launched from North Island in the Seychelles and from Diego Garcia in the Chagos

Archipelago might serve to determine the moment of onset of the southwest monsoon regime over the central parts of the Indian Ocean. It would be useful if an aircraft could be stationed in the area for several months to measure the air–sea interactions. The aircraft should be equipped with a high-precision, inertial-navigation system that would act as the basic reference for gust sensors. With such a system the turbulence and the turbulent fluxes could be measured over a spectral range extending from the inertial subrange to mesoscale disturbances. Certainly, aircraft are capable of getting useful data for limited times in regions that are otherwise practically inaccessible.

The southwest monsoon over the Indian Ocean, more than any other major weather phenomenon in the world, affects the crops, fisheries, and economy of the largest number of people in a developing area of the world. This population is utterly at the mercy of the southwest monsoon, and it is well known that the rains vary from year to year. Increased knowledge of the southwest monsoon that could lead to a significant ability to predict its behavior would be a very real contribution to a large number of people living a precarious existence.

We therefore recommend that a group of interested oceanographers and meteorologists formulate a detailed plan for a five-year study of the monsoon circulation in the western Indian Ocean and that they clearly establish the data requirements for such a study.

In this region, as in the Equatorial and South Pacific, a strong case can be made for maintaining one or more resident ships. In the Pacific, the long distances involved suggest a ship of unusual speed and endurance, while a more conventional ship would be adequate for the Arabian Sea. In both regions, local research facilities are modest, and oceanographic capability must be imported. A resident ship could provide the means to make the necessary observations over a suitably long period of time, could serve as an economical platform for visiting investigators with interest in the region, and could provide valuable training and experience for local investigators. Previous experience with such ships should be drawn upon to help in determining the most effective manner of organizing and conducting such an operation.

Western Pacific and China Seas

The wintertime meridional circulation of the atmosphere is stronger over southeast Asia and the Western Pacific than anywhere else. No physical barrier is interposed between the Siberian cold pole and the equator, where mountainous islands powerfully aid convection of deeply moist air.

During those periods, sometimes lasting weeks, when the northeast monsoon is strong and it cools the surface layers of the China Seas more than usual, latent heat of condensation released aloft over Indonesia is efficiently drained away northward. Then, the subtropical jet stream intensifies, and extreme weather develops downstream over the Pacific and North America. At other times, heat disposal is inefficient, and the monsoon and jet stream are weaker than normal.

Already, meteorological and oceanographic data are sufficient to permit preliminary quantitative studies to be made of these important ocean–atmosphere changes.

We recommend that simple numerical models be devised and tested with available data to determine the relative importance of the various energy terms in this circulation system, preparatory to designing a detailed special observational program.

Equatorial Pacific

An excellent region for early study is the tropical Pacific, where several components of the equatorial-current system change their strength, width, and latitude with season. Three regular north–south hydrographic sections could be made monthly along selected meridians from latitude 20° N to 20° S, for a period of 6 to 8 years. The same ships could be used to tend a dense set of weather buoys set out along the 165th W (or other meridian). It is very well documented that long-period changes can be observed on such a section: there are remarkable long-period, nonseasonal changes on the equator in contrast to regular seasonal variations in temperature and salinity of the surface at higher latitudes.

In a narrow belt along the equator, upwelling takes place whenever and wherever the wind component from the east surpasses a critical speed of a few meters per second. Long periods of uninterrupted easterly winds lead to a widening of the coldwater belt toward 5° N and 5° S. Such a wide coldwater tongue along the equator is a quasipermanent feature of the Eastern and Central Pacific. Canton Island, 2°48′ S, 171°43′ W, is influenced by the upwelling water more than half the time of its sea-temperature record. Relatively strong and cool easterly breezes accompany the cold surface water. The relatively shorter periods of absence of upwelling water at Canton Island occur with light doldrum winds of variable direction. The dynamics of these climatic oscillations may be tied to a thermal circulation in the atmosphere from the cold, equatorial, Eastern Pacific to the warm Western Pacific with a probable return by way of the upper troposphere. The pressure variations here referred to may be the same that Sir Gilbert Walker discovered and tried to explain as a result of air exchange between the Indian and Pacific Ocean areas.

The dominant role of the Equatorial Pacific in maintaining far-reaching atmospheric teleconnections must be related to the combination of large amplitudes of interannual temperature variations and the large size of the equatorial area within which they occur. The Equatorial Atlantic is in that sense much less influential in the remote control of extratropical weather. The large climatic anomalies that do occur in middle and high latitudes of the North Atlantic sector seem to be due to a combination of the variable thermal input to the atmosphere over the Gulf Stream and of the longitudinal spacing of ridges and troughs in the upper tropospheric westerlies, which, in turn, are dependent on the spacing and intensity of such systems over the North Pacific.

We recommend that oceanographers and meteorologists interested in the phenomena of the Equatorial Pacific formulate a detailed plan of cruises, buoy deployment, use of ships of opportunity, and island observatories, including a sufficient number of upper-air stations to elucidate all features and processes involved in the large-scale, long-period ocean–air interaction. The experience so gathered would be an important preliminary to eventual expansion of prediction models to include the entire atmosphere and ocean.

Subtropical Upwelling Region

A fourth regional study of immediate concern should be made in a selected subtropical upwelling region. Oceanographers and meteorologists should agree on a choice of area—preferably one that is interesting to biologists and fisheries scientists—and design a proper study.

PERMANENT OCEAN STATIONS

We hope to encourage establishment of more midocean time-series observations. There are very few of these at present. Important time series are maintained in the Northeast Pacific at Weather Station PAPA, and near Bermuda on the *Panulirus* stations. Since 1967, time-series observations have been made routinely at all six ocean stations maintained by the United States. It seems very important to extend the sampling at these stations to include more geochemical determinations. Although much work is involved, a fuller program of measurement on a regular basis is called for. For example, regular sampling of tritium at

Bermuda would probably shed light on rates of formation and spread of the 18° water in the Sargasso Sea, and indirectly on the dynamics of Ekman convergence, among other things.

The importance of time-series observations from such stations lies in the fact that the atmospheric activity that leads to major variations in coastal areas (as well as in the open ocean) appears to be generated frequently by complex, large-scale air–sea interactions in midocean. For example, warm-season upwelling off California depends largely on the strength of the Pacific High, which in turn is coupled with conditions at the underlying sea surface. Investigation of this relationship will be aided by the experimental network of nine buoys recently established in the mid-Northern Pacific. This North Pacific Buoy Program should also provide useful information on the relative merits of ships and buoys as measuring platforms.

Islands are probably the most economical platforms for air and sea measurements in the ocean. Future meteorological programs will involve a considerable expansion of upper-air stations on remote oceanic islands. The very important station on Canton Island has recently been closed down entirely, which brings to an end a very important established time series and is a serious loss to science. The reason for closing down Canton Island was largely an economic one: the station costs are $350,000 per annum, but there is also involved the matter of frequent transportation and quick access to medical and other essential facilities. If a substantial number of new island stations are to be manned in the same way (including a re-established Canton Island), the cost will be high. It may be possible to man such stations with local inhabitants, through contracts with local authorities—for example, a Gilbert Island observatory could be manned largely by the Gilbertese. Consideration should also be given to the development and use of automatic recording or telemetering equipment.

It should be possible to measure oceanographic variables other than sea-surface temperature and salinity at GARP stations on oceanic islands. Can one detect the direction of flow of large-scale ocean currents around islands by electrical potential measurements from electrode arrays or by other means on islands or in the lagoons of atolls? If so, this would be a powerful and convenient technique for monitoring currents from islands without having to conduct seagoing, ship-borne operations from each island.

Some island stations should be retained for long periods extending beyond the Decade. In particular, long-period solar-radiation measurements are needed. Without these, it will be difficult to understand long-period variations in either sea or air, or their interaction. At least one oceanic or atoll tropical station is required to monitor net radiation, incoming and outgoing, and to obtain good cloud records.

The *Panulirus* stations offshore of Bermuda enable us to monitor the changes in the main thermocline and seasonal thermocline of the center of the subtropical gyre of the Atlantic Ocean. For example, it now seems quite likely that there are major differences in the heat storage there from year to year. Since these are doubtless related to major changes in the air–sea interaction over the central Atlantic, detailed solar-radiation measurements are desirable; unfortunately, during the past thirteen years of the life of the time series, they have not been taken. The variety of measured variables at Bermuda is gradually being increased, with the intention of building a fully instrumented island observatory there. For example, a series of internal wave arrays is projected. Inasmuch as an instrumented tropical atoll is also desirable, as preparation for TROMEX (Tropical Meteorological Experiment, scheduled for the early 1970's in the Marshall Island area), a more detailed observational program at some Pacific atoll should be undertaken. Existing programs at Fanning Island or at Kwajalein might be expanded. The Gilbert Islands have also been proposed as a possible site because these islands are close to the Equatorial Undercurrent, and their use would facilitate frequent measurement of this important current.

PILOT STUDIES OF NEW METHODS OF MEASUREMENT

Transport of Major Currents

Certain new methods of measurement are of potential use in developing a global ocean-forecasting system. They have to be tried out to see how feasible they are. Thus a new method for direct measurement of transport of major currents has been developed in which the horizontal displacement of a freely falling device is measured. This method requires extremely precise navigational control and continuous employment of a ship. It might be necessary to assign two research vessels to occupy each section across a current, and it will be necessary to gain additional experience with the method before a clear judgment can be made about the practicability of incorporating such monitoring in a global-forecast scheme.

Other possible methods for monitoring transport include the measurement of sea surface slope by geodetic satellite (in the very early stage of development), measurement of electric potential across straits, tracking of drogues or drifting buoys (possibly by satellite), current measurement from fixed buoys, and the estimate of geostrophic transport from the distribution of mass.

Each of these methods may have serious drawbacks and limitations, and it is necessary to determine how useful they will be. There have already been many observations over the years across the Kuroshio and Gulf Stream by some of these methods. A critical review of past results could clarify the question of whether more systematic effort along these lines is desirable.

In most parts of the ocean, for the foreseeable future, the geostrophic method appears to be the most suitable for routine use. The distribution of mass is a function of the distribution of density, and hence of temperature and salinity. Given a knowledge of density distribution, it is possible to use the geostrophic approximation to obtain an estimate of the distribution of relative velocity. Absolute velocity, and hence absolute transport, can be determined only if an appropriate reference surface (or level of no motion) can be established. This is often not possible. However, by using an arbitrary reference, one can make meaningful estimates of changes in velocity and transport. But one should not be too sanguine about this.

A cautionary tale can be told about such "standard" sections across ocean currents. Recently it was debated whether Antarctic scientists should institute a time series of hydrographic sections across the Antarctic Circumpolar Current, with the idea that large seasonal changes in transport or position might be found. It is nearly impossible to make a judgment about such a question "in vacuo," but fortunately there exists a series of 16 sections made by *Discovery II* in 1938–1939 south of the Cape of Good Hope. Although these were never worked up, they provide a series of data to serve as a test case. When these sections were analyzed, no discernible difference could be detected in water beneath 1,000 meters, even though the great slope of the isotherms at depth indicates a strong current all the way to the bottom. The spacing between stations was approximately 3° of latitude, and it is possible that with closer station spacing some definite variability might have been detected. The fact remains that the main feature revealed by the particular sampling program undertaken by *Discovery II* was essentially a very steady current; any fluctuations were below the noise level.

We may expect that various proposals will be advanced from time to time to monitor transports through the Bering Straits, the Straits of Gibraltar, the Drake Passage, and so on, and we must plan to be able to judge these proposals realistically. Thus the present studies of intercomparison of cable methods and free-instrument methods and of the geostrophic method in the Straits of Florida are important to encourage. Is there any way of "calibrating" the geostrophic method, as has been sometimes suggested? Will limited numbers of moored instruments on buoys determine what we want to know about these great currents—for example, will they evaluate Reynolds stresses significantly? And again, is there a field of application here for the new expendable bathythermograph (XBT) dropped from aircraft and ships of opportunity?

Moored Current-Meter Arrays

Because of the significant energy content of the velocity field at high frequency, serious sampling problems arise in the design of an effective ocean-monitoring system. In order to evaluate these problems, it is necessary to establish cooperative studies of the technique of current measurement from small grids and arrays of moored current meters. Studies of the current spectrum in the range of 0.1 to 100 cycles per day should result in fundamental information on the propagation of energy in internal, inertial, tidal, and planetary waves. Already, preliminary attempts have been made to intercompare velocity-measuring devices used in a number of countries, and pilot experiments with small arrays are being conducted by several East Coast laboratories. We therefore recommend that investigators in the Atlantic join efforts in coordinating this work and in planning further experiments with buoy arrays. These experiments are a necessary step toward the eventual design and establishment of an ocean-monitoring system.

Bottom-Pressure Devices

Instruments for measuring pressure fluctuation at the bottom of the ocean hold great promise for monitoring changes in oceanic conditions. They are as fundamental to synoptic oceanography as the familiar barometer has been to meteorology, but technically they are much more difficult to develop. Prototype ocean-bottom-pressure gauges are being constructed at a number of institutions both in the United States and abroad. Oceanographers need an opportunity to use them and to study the results. Therefore, we think that special programs designed to use them to study tides and other bottom-pressure changes are highly desirable. To begin with, attention will undoubtedly center on the study of tides in the deep ocean because tides constitute the largest signal and because they are the most easily understood.

An international group of scientists has been working together since 1965 and has proposed to organize systematic measurements and analyses of deep-sea tides in the world ocean. The objective is to construct global cotidal charts and to apply them to various geophysical problems, such as estimation of dissipation of tidal energy through friction, the transition problem connecting deep-sea tides and those on the shelf, interpretation of other geophysical measurements through a proper correction for global ocean tides, inference of the earth's interior stress field, analysis of barotropic signatures of passing storms, and dynamics of the bottom boundary layer of the ocean.

The problem of tidal transition between the shelf and deep water is one of particular importance and difficulty. However, it appears that a number of relatively shallow instruments for use on the shelf are being developed, and during the next few years it seems likely that various nations will be making an intensive effort to investigate the transition problem off their respective shelves.

In the deep ocean, measurements would be made by bottom-pressure recorders capable of making hourly measurements of tidal height to the nearest centimeter in depths up to 6 km. It is proposed to make month-long measurements at each of some 300 stations in various parts of the ocean. The location of these stations is being studied. In part, the measurements can be made by ships of opportunity, in part by tidal expeditions devoted primarily to such measurements. In either case, the instrument is planted on the sea floor and recalled to the surface at a later date. There is no need for the stations to be made simultaneously because the tide-producing forces are known. Where several instruments are recording simultaneously, however, time variations in the pressure differences between stations will give information on variations in barotropic currents. It seems likely that the experience gained in the new technique of bottom-pressure measurement will lead to useful data for inclusion in a global ocean-forecast system and that various arrays and variations of these instruments will eventually become an important part of the global monitoring network.

THE DEEP OCEAN

It would be a mistake to limit the activities of the IDOE entirely to surface layers. Understanding of the mechanism and rate of formation of deep waters and their transport and exchange between basins and between oceans is an important element in the over-all understanding of how the ocean works. Interactions of the deep ocean with the surface layer can have important implications for primary production and fisheries; they also relate to the alteration of surface conditions affecting the atmosphere. But much of the deep ocean remains to be explored to establish its average state.

A basic set of high-quality observations of temperature, salinity, dissolved oxygen, and nutrients between surface and bottom is a first requirement. Only since 1954 has a suitably precise method for determining salinity been available. Many suitable deep stations have been made since 1954, but there are still large regions from which no data are available. The most conspicuous gaps are the Weddell Sea, the Arctic Ocean, southern parts of all oceans, and some parts of the North Indian and Pacific Oceans. There are two views toward how these new stations should be distributed. The first view is the optimistic one: a suitable grid might be to have one such station per unit area of 30×10^4 km^2 (a 5° square near the equator). About 1,000 stations would then suffice; at most, half of these areas contain modern, high-quality deep observations. It should be possible to sample the rest during the Decade, in large part from cruises with some other primary mission. Since observers from a variety of institutions and countries might be involved, it is essential that standardized techniques of high quality and known accuracy be used. Special effort will be required to make the observations in high latitudes, particularly in winter.

The reason that this disposition of stations is properly called the optimistic view is that the number of stations proposed is the absolute minimum. General oceanographic experience of the past indicates that isolated single stations are of limited use and that regular dense sections across major ocean basins, such as were carried out in the Atlantic during the International Geophysical Year, are highly desirable. This is the second view, the pessimistic one: namely, that there is no use shirking the work of making good sections across all major ocean basins.

The two principal regions of deep- and bottom-water formation appear to be the Weddell Sea and the region off southern Greenland. Winter observations in these regions are required to understand the sinking mechanism and to ascertain its variations in time and space. Both ocean and atmosphere are involved and must be measured. It is extremely difficult to make complex and precise measurements in these regions from surface vessels during winter, and serious consideration should be given to using a suitably equipped submersible.

Completion of the desired net of deep stations would permit a quantitative inventory of volume and distribution of all water masses of the oceans and would provide a base line for future studies of secular change in the deep ocean. It is also desirable to obtain direct measurements of flow of deep waters. This is difficult to do on a global basis, since the deep currents may be sufficiently variable that prolonged measurement is necessary. There are, however, indications that higher speeds and more steady flow occur in the deep channels connecting ocean basins, particularly on the western sides of oceans. Measurements of these flows are now possible, for example, with unattached bottom-tethered current meters. If such measurements were made in the various passages connecting the several deep basins of the Atlantic, Indian, and Pacific Oceans, and the channels through which the Antarctic Circumpolar Current flows, it would be possible to make estimates of the balance of water, salt, heat, and dissolved substances.

SOURCE AND SINK

Geochemistry and Environmental Change

INTRODUCTION

Until recently, studies of the chemical properties of seawater were conducted primarily as an adjunct to physical and biological oceanography. The great postwar boom in geochemistry brought on by the discovery that isotopes could be used to tell time and temperature brought forth a new generation of chemical oceanographers who recognized the great wealth of information tied up in the chemical and isotopic properties of the sea. Over the past decade they have developed the techniques needed to unlock this information and have made limited surveys to demonstrate the potential of their ideas. These investigations have demonstrated that the systematic and coordinated study of the chemical and isotopic properties of the sea is essential to our understanding of the sea. Geochemical investigations can contribute to enhanced utilization of the ocean and its resources in the following ways.

Disposal of Wastes

Most of man's wastes will ultimately find their way to the sea, where they may jeopardize the living resources. To evaluate this hazard, we must know more about the means by which chemicals are carried from place to place in the sea. This involves, first of all, improving our understanding of how the ocean mixes. The complex maze of advective and diffusive motion must be disentangled. Without using information available from the distributions of the various natural and man-made radiotracers, it is doubtful that this can be done. Chemical compounds are not only transported along with the waters, but many become attached to particles that sink under the influence of gravity and may then be oxidized or dissolved in the deep sea. These adjunct pathways must also be understood if we are to predict the future distribution of pollutants.

Oceanic Productivity

Food supply from the sea is ultimately limited by the availability in surface waters of nutrient elements such as phosphorus and nitrogen. Although these substances are abundant in the deep sea, the surface waters of the oceans are usually nutrient-poor. As upwelling brings them up from the deep, nitrogen and phosphorus are quickly removed by organisms. Someday man may wish to increase the productivity of the sea by artificially accelerating the rate of vertical mixing. This will be possible only when the present modes and patterns of mixing are understood. Again, it is the distribution of radioisotopes that holds the key.

Climatic Change

There is a suspicion that man is inadvertently changing the energy balance of the planet earth by the combustion products of fossil fuels. Solid suspensions and carbon dioxide change atmospheric transparency and hence the heat balance of the globe. The former scatter incoming radiation, the latter absorbs outgoing infrared radiation. It is as yet uncertain which effect is gaining the upper hand. If the suspensions prevail, the globe would cool; if the carbon dioxide accumulates, it would heat up. There will also be secondary effects on cloud cover and water vapor content of the atmosphere. Disturbance of the climatic equilibrium could have serious consequences for the earth and its biota. The oceans are the major sinks for washout of solids and for solution of the carbon dioxide. The residence times of these heat absorbers in the atmosphere are presently not well known. Especially for the carbon dioxide cycle a detailed knowledge of ocean chemistry and mixing is essential.

Conditions at the sea surface must be considered in the eventual attempts to improve the climate. The transfer of heat and moisture at this interface plays an exceedingly important role in the distribution of rainfall and temperature. The application of chemical tracers to studies of air–sea interaction is in its infancy, but the information already available from isotopic, chemical, and radiochemical studies suggests that chemical oceanography will make an important contribution to this field.

Mineral and Petroleum Resource Formation

The processes of formation of the petroleum found recently in the sediments on the floor of the Gulf of Mexico, of the metal-rich deposits forming from the hot brines on the Red Sea floor, and of the nickel-bearing manganese nodules that cover much of the deep-sea floor are poorly understood. Although much can be done to exploit these resources by geological and geophysical prospecting, the advantages gained by understanding the mechanisms leading to their formation are self-evident. Also, it may prove possible to detect the proximity of certain resources by looking for chemical anomalies in the overlying water masses. Because the distributions of trace metals and hydrocarbons in the sea are virtually unknown, the search for the hidden resources of these materials should benefit substantially from the proposed chemical survey.

GEOCHEMICAL OCEAN-SECTION STUDY

The limited reconnaissance investigations of the past decade have demonstrated the potential of geochemical methods in oceanography. The need is now apparent for a much more extensive and systematic sampling of appropriate chemical properties throughout the world ocean. Accordingly, we propose a Geochemical Ocean Section Study (GEOSECS).

Briefly, the plan is as follows: Three long traverses would be run from the Antarctic to the northern limit of the western basin of each of the major oceans. The tentative locations of these tracks is shown in Figure 1. At each of the 120 stations, temperature and salinity would

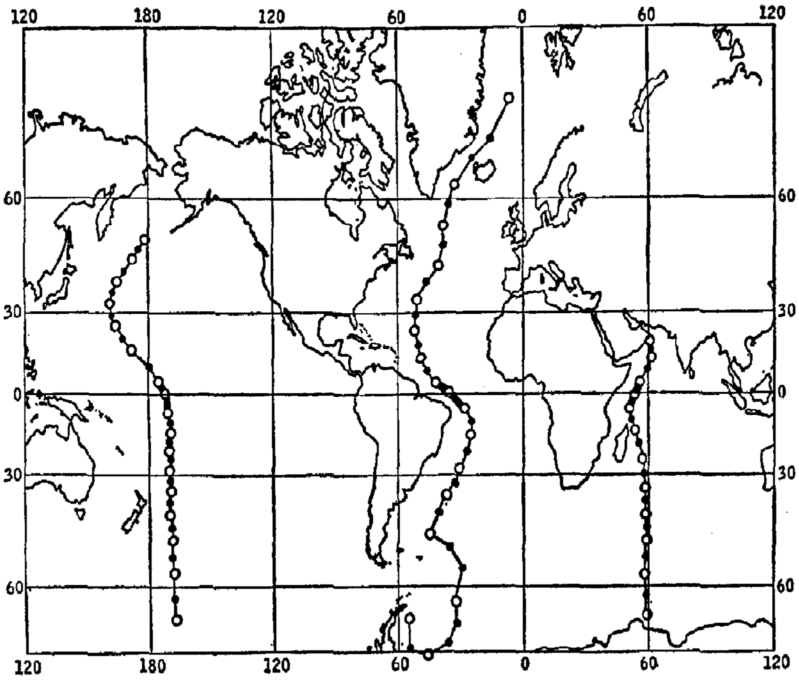

FIGURE 1 Tentative location of tracks of 120 geochemical sampling stations proposed for 1970–1972.

be measured between surface and bottom, and approximately fifty 30-liter water samples would be collected. These samples would be analyzed for dissolved O_2, nutrient compounds (of P, N, and Si), ΣCO_2, pCO_2 alkalinity, pH, $^{13}C/^{12}C$, $^{18}O/^{16}O$, D/H, trace metals, tritium, radon, and rare gases. At each of 60 stations, twenty 1,000-liter samples would be collected for analyses of rare earths, ^{14}C, ^{228}Ra, ^{226}Ra, U, Th, ^{90}Sr, Pu, and ^{7}Be. At the same stations, *in situ* extractions of certain radiochemical species and particulate matter and solubility experiments for calcite, barite, and opal would be performed. All stations made during the geochemical survey would contribute to the basic set of deep observations discussed in the preceding chapter.

For the entire survey, about 300 days of ship time will be needed. It will be most convenient, and problems of standardization of methods will be minimized if no more than two or three ships are used. It is important that participating ships not be radioactively contaminated. Because of the multiple analyses to be made, large scientific parties will be necessary, and it is estimated that the full participation of all marine chemistry groups in the United States will be required to carry out the program.

During the summer of 1969, a test cruise off Hawaii is planned. This will permit tests of sampling procedures and of the reliability of measurements, and it will facilitate planning and design of the survey. It is hoped that the long traverses could begin in November 1970 and be completed within two years. The analyses of samples would require an additional two years.

TRANSPORT PROCESSES

Another proposed geochemical program concerns the study of transport processes from the continents. Quantitative data on the amount of materials produced in the major weathering cycles and subsequently introduced into the marine environment have not been gathered systematically so that mass balance calculations can be made. Further, the industrial, agricultural, and social activities of man result in the introduction of materials, some alien to the oceanic systems, in amounts comparable to natural inputs. The aim of this program is to ascertain these rates of transfer for chemical species, which is important for the elucidation of natural processes in a quantitative way and for the definition of the potential effects and alterations upon the marine environment by the man-introduced contaminants.

The monitoring of the river-borne chemicals includes three groups of substances: the suspended particulates, the dissolved species, and the bed-load. The major rivers of the world should be sampled at appropriate intervals (perhaps monthly) to take into account the various stages of the river flow and should be continuously monitored for flow rate and salt content. The river-water samples would be taken in sample sizes of the order of 50–100 liters to allow for a host of chemical analyses and operations. Such temporal parameters as pH and CO_2 should be analyzed immediately after recovery of the samples. Separation of the sample into dissolved and particulate categories can be effected either by centrifugation or by filtration techniques—the method to be agreed upon by a consensus of workers in the field. Analyses by agreed methods should be performed upon such major constituents of river waters as Na, K, Mg, Ca, total Co_2, SO_4, Cl, and those minor species whose concentrations are of significance to geochemical computations. Man-introduced species such as the pesticides, mercury and lead, as well as such chemicals of industry and agriculture as nitrate and phosphate and the halogenated organic compounds used in the chemical industry, should also be assayed.

Atmospheric inputs should be monitored from the three principal global winds systems—the trades, the jets, and the polar easterlies.

Direct air sampling and analyses of rains can be used to find the instantaneous or differential inputs of suspended dust particles into the oceans. Every effort should be made to improve existing air-sampling techniques for volumes of a million cubic meters or so. Present techniques involving glycerine-coated nylon nets are inadequate and poorly characterized for collection efficiency.

The suspended loads and bed-loads of the rivers and the suspended particles in the atmosphere are to be subjected to x-ray diffraction analyses for mineral determinations and size determinations, either by settling techniques or by direct measurement. Emphasis should be placed upon the clay minerals, which can be especially diagnostic of source, and upon the minerals such as talc used as a carrier for DDT and industrial soot, which have been introduced by man.

The atmosphere and rain samples can be supplemented with permanent snow-field samples for the assay of atmospherically transported solid phases over the past 100 years or so. These glacial sediments can often be dated by stratigraphic or radioactive techniques. They will provide an especially important base line to study the input of contaminants by man to the ocean via the wind systems. The glaciers are especially attractive for this type of investigation for they exist at all latitudes and hence receive inputs from the three global wind systems.

We realize that a program such as this will be successful only if hydrologists, geomorphologists, sanitary engineers, and biologists work closely with geochemists. Representatives of these adjunct fields should be included in all discussions of how these programs should be carried out.

WAYS AND MEANS

In the introductory section, we discussed the basic goal and distinguishing features of the International Decade of Ocean Exploration (IDOE), objectives of U.S. participation, and some uses of the ocean that could benefit from investigations conducted during the Decade. In the program sections, the nature and scope of appropriate Decade studies were described. Now it remains to consider ways and means whereby the goals of the Decade can be achieved.

From one point of view, U.S. participation in the Decade can be considered as an enlarged national program. The extent to which the national program can be enlarged depends on the adequacy of available funds. The implementation of such a program also depends on the following elements:

1. Development of enhanced capabilities (manpower, facilities, and improved technology)
2. Establishment of program-management procedures (planning, coordination, and review)
3. Encouragement of associated social studies

To the extent that the international program is an integrated set of national programs, each participating nation will have similar problems to consider. In addition, implementation of the international program involves other considerations:

1. Strengthening possibilities for cooperation (freedom of scientific research and technical capabilities of developing countries)
2. Improvement of international coordinating apparatus

The details of such matters must be examined by national and international planning and administrative staffs. However, preliminary consideration is given here in order to suggest a framework in which realistic program proposals can be developed.

ADEQUACY OF FUNDS

We have not attempted to estimate in any detail the U.S. funds required to prosecute successfully programs of the scope described in this report. Such a calculation demands a more careful analysis than was possible in the time available, and must also await elaboration of program details by those who intend to carry them out. However, it is our opinion that a significant increase in expenditure will be required for upgrading present facilities, for developing new capabilities in preparation for Decade programs, for conducting fieldwork, and for analyzing and publishing Decade results.

The National Council on Marine Resources and Engineering Development has estimated that about 30 percent of the present U.S. federal marine science budget (as defined by the Council) is allocated to programs related to ocean exploration. If the Decade were promulgated with insufficient new funding from governmental and private sources, participating activities would be forced to reprogram to meet Decade obligations, to the almost certain disadvantage of their essential regular activities. It seems unlikely that a Decade conducted in this way would have a significant impact on the goal of enhanced utilization of the ocean and its resources.

At this early planning stage, it is difficult to specify the additional funds required. In the discussion that follows, we express our views on the limits between which Decade expenditures might fall. The sums are not derived from a detailed breakdown of anticipated costs, but are only gross estimates reflecting the experience and judgment of the *various members of the Steering Committee who participated in the discussions, and they should be considered in that light. Only when program details have been elaborated will it be possible to arrive at a more realistic picture of the cost of an effective* IDOE.

It is our opinion that if much less that $100 million of new money per year (averaged over the Decade) can be made available for the U.S. program of ocean exploration as described in this report, it would be undesirable to identify the set of programs as an International Decade of Ocean Exploration. If as much as $500 million of new money per year (averaged over the Decade) can be made available, we believe that essentially all the programs described in this report can be implemented. To the extent that available funds fall between these limits, more or fewer of the Decade objectives can be met.

In order to examine the gross features of the pattern of Decade expenditures, the assumption was made that a total of $3.5 billion of new funds would be available over the life of the Decade. A rough division of funds among the classes of operating entities might be as follows (amounts in $ millions):

	Upgrade	Prepare	Implement	Total
Academic	100	200	500	800
Government	100	500	1,200	1,800
Industry	50	350	500	900
Total	250	1,050	2,200	3,500

The terms in this and the following table are used in the following sense:

Plan: conduct conceptual studies, establish priorities, allocate tasks, develop program plans and budget, establish management and coordination mechanisms (national and international).

Upgrade: replace, modernize, and augment, as required, the present investment in buildings, platforms, installations, equipment, and personnel.

Prepare: initiate the necessary research, development, and capital-investment programs to develop the new capabilities required for Decade programs.

Implement: conduct the exploration effort and related research activities adopted as Decade programs, and analyze and disseminate their results (Table 2).

According to this scheme, emphasis during the first three fiscal years

Table 2 A Possible Spending Pattern for Implementing Decade Programs (in millions of dollars)[a]

Fiscal Year	Expend	Upgrade	Plan	Prepare	Implement
1971	150	X	X	x	x
1972	250	X	X	X	x
1973	350	X	X	X	X
1974	450	x	X	X	X
1975	500	x	x	X	X
1976	500	x	x	X	X
1977	400	x	x	x	X
1978	350	x	x	x	X
1979	300	x	x	x	X
1980	250	x	x	x	X

[a] The symbol X indicates major emphasis during the indicated year; the symbol x indicates minor emphasis.

(1971–1973) would be on upgrading present facilities and on intensive planning. Major development of new capabilities would begin in fiscal year 1972. Although some programs should be started as early as the first or second year, large-scale implementation of Decade programs would begin in fiscal year 1973. During the early years of the Decade, programs described in the present report might be emphasized; by the midpoint, some of these programs will have been completed or dropped, and other programs will have been conceived and initiated.

ENHANCED NATIONAL CAPABILITIES

In considering the required capabilities, it should be noted that these are specialized only in part. Ocean research is conducted by people using equipment in laboratories ashore or at sea. Many scientists and engineers engaged in marine investigations were trained in fields other than oceanography, and many of the laboratories, research platforms, and items of equipment were not specially designed for ocean purposes. In the following sections, this dualism is recognized, and special attention is given to the more specialized requirements.

Manpower

Any significant expansion of national activities in ocean exploration will increase the demands for manpower in this field. At the subprofessional level, the requirements are not highly specialized and can often be met by transfer from related fields. However, marine technicians present a special case where specialized training is advantageous. Under the Sea Grant Program, there are now a number of training activities designed to meet this demand.

At the professional level, about half of those people now active have received formal education in some marine-related discipline; the others have transferred from other fields. Such "immigrants" are particularly common in fields such as engineering where, until recently, there have not been widely available suitable graduate curricula for specialized study. Specialists in the systematics of marine organisms are often educated in zoology departments; many more systematists than are now available will be required for the biological programs of the IDOE. The variety of professional backgrounds among ocean workers is likely to continue, and it is desirable for several reasons. At a time of expanding programs, the supply of manpower can be increased more rapidly if not all the people involved require extensive special training. The injection of new ideas and experience from other fields serves to regenerate marine research and to keep its standards high. To a limited extent interdisciplinary interaction may stimulate marine resource utilization. At the same time it must be recognized that an adequate over-all supply of scientific and engineering manpower must be available if the ocean field is to succeed in attracting the people it requires.

Yet there will continue to be a requirement for an increasing number of scientists and engineers educated in marine specialties. These are needed both for teaching and to provide the special skills and knowledge of the oceans essential for planning and conducting realistic ocean programs. During the last decade there has been a significant growth in the number and size of schools offering such education. There have been estimates that this increased capacity is adequate to meet the anticipated requirements during the next decade. However, because of the long time required for specialized education and training, it is important that there be continued growth in the number of students in these schools. The demand for specialists must be kept under continuing review as programs for the IDOE are developed so that action to increase the supply can be taken as necessary and in timely fashion.

FACILITIES

The facilities required for implementing ocean-exploration programs include buildings, for teaching, research, and services; platforms (ships, aircraft, buoys, and towers); special installations (data centers and instrument-testing facilities); and associated equipment. To some extent, these elements relate to improved technology where they are more properly discussed; in this section, we will treat the more general aspects.

Buildings

During recent years, a vigorous building program has met many of the most urgent laboratory requirements. Yet an obvious characteristic of most active laboratories is that they are becoming space-limited. The problem is particularly acute in academic laboratories where non-federal funds are required for part or all of the building costs. Shortage of space impedes the development of research and of teaching because space requirements per graduate student are relatively large. The Decade will result in a significant increase in the number of personnel engaged in ocean exploration, research, and development. Therefore, an analysis of building requirements should be made at educational and research centers where Decade programs are to be conducted, and early priority should be given to obtaining the required space.

Platforms

Although a number of elaborate and advanced platforms are discussed in a subsequent section of this report, the major burden of exploration during at least the early years of the Decade will continue to be carried by surface research and survey vessels.

An impressive number of ships has been built during recent years, yet much of the fleet of the academic laboratories still consists of antiquated conversions. Because of the manner of funding construction of replacement vessels, there is no general plan for upgrading this important collection of vessels. The present restriction in funds will inevitably delay construction and availability for Decade work of ships already required by these laboratories. We recommend that a long-term interagency plan be developed for analysis of the ship requirements of academic laboratories and that necessary funds be provided for upgrading the academic fleet.

Government laboratories have been reasonably successful in obtaining funds for new ship construction but have been less fortunate in financing the equipping and operation of these vessels. Some new research and survey vessels are being laid up on delivery; others are operating on limited schedules or at reduced efficiency because of the lack of essential equipment. Full activation of these vessels will be required if the agencies concerned are to play a substantial role in Decade programs. We recommend that the research and survey vessel building program of government agencies be continued and that funds be provided for full operation of available ships.

There is now a substantial fleet of industry vessels engaged in various aspects of ocean exploration. In view of the flexibility of funding and decision-making in industry, it is likely that this fleet can be expanded or modified more rapidly than those of the academic or government laboratories. The availability of suitable research and survey vessels will not be a major impediment to industry participation in the Decade.

Special Installations

There are several specialized national facilities that are considered essential for a national oceanographic program at the present level as well as at the expanded level foreseen for the Decade. These include facilities for the processing, analysis, storage, and retrieval of ocean data, both historical and in real time, and facilities for the testing and calibration of instruments and for the provision of measurement standards. Prototypes of these facilities exist in the National Oceanographic Data Center, the Smithsonian Oceanographic Sorting Center, the newly formed National Oceanographic Instrumentation Center, and the National Bureau of Standards. The technical aspects of these requirements are discussed subsequently. Pressures generated by Decade programs will make more urgent the broadening and strengthening of such facilities.

Associated Equipment

The increased cost of improved equipment should be noted here. The satellite navigation system is not only more precise than the sextant but is more expensive by at least two orders of magnitude. Similar in-increases can be cited for measurements of salinity (by *in situ* salinometer) and nutrient substances (by autoanalytical equipment) or for computation (by digital computer). Even though examples can be found of lowered costs for certain kinds of equipment, it appears that the cost of equipping existing and new facilities for ocean research is significantly higher than it was ten years ago. Although operating costs have climbed, as a result of increased wages and inflation, there is no doubt that in some cases the improved equipment can lead to economies in the use of manpower as well as giving more and better information. Yet it is difficult to see how the proposed Decade programs can be realized without significantly increased expenditures on equipment.

IMPROVED TECHNOLOGY

Important technological advances will be required in order to carry out the programs of Decade. These developments in equipment, methods, and techniques will be made in private and government laboratories. When the goals and programs of the Decade have been more clearly identified, the required technological developments can be established. Some will have more widespread application than others, and priorities will have to be decided. Adequate funds must be allocated to the selected development projects, and their products must be made widely available to Decade participants. Examples of technological advances known to be required are discussed in the following pages.

Navigation

One of the problems that has plagued oceanography from its beginning is the difficulty of establishing position above, on, and beneath the ocean surface. The requirements for position accuracy differ from place to place and from problem to problem. In the case of geophysical measurements on the continental shelf, a precision of a few feet may be required, whereas for certain biological measurements in the open ocean, an uncertainty of several miles is acceptable. There are also studies, such as the measurement of surface currents, where high relative precision, of the order of tens of yards, is required, but where accurate knowledge of the absolute position is unnecessary. Certain geophysical studies, such as gravity measurements, also require precise navigation in order to determine the Eötvös correction, which allows for the gravitational effect of the differential movement of the platform relative to that of the rotating earth.

In general, oceanographers have used navigational systems developed for other purposes. Until recently, most ocean exploration was based on celestial navigation; in certain regions since World War II electronic systems such as Decca and Loran have become available. Yet vast expanses of the ocean, particularly in southern latitudes, are not served by such navigational aids. As new systems have been developed for other purposes, and when their price has not been prohibitive, they have quickly been adopted; examples include Omega, VLF, Hyfix, and Shoran. In addition, oceanographers have developed their own methods of precise relative navigation, such as the use of taut-wire moorings and radar, or of acoustic transponders on the sea bed.

New possibilities have opened up with the perfection of a navigational satellite system and its availability for nonmilitary use. On July 29, 1967, presidential approval was announced of a recommendation that the Navy's system be made available for use by civilian ships and that commercial manufacture of the required shipboard receivers be encouraged. These receivers are already in use on a few research vessels.

The development of an International Decade of Ocean Exploration imposes the need for a global system available to all participants. An ideal system would include the following characteristics: global coverage, common geodetic datum, constant availability, unambiguity, absolute accuracy of at least 200 m, capability of random access, self-monitoring, relative simplicity at the sensor, capability of providing different ranges of accuracy, and availability to infinite number of users.

This ideal is most closely approached at this time by the satellite system in combination with the VLF OMEGA system, which can provide relative control in between periodic absolute position fixes. Further development is desirable, however, and the following properties might be specified:

It would be a synchronous satellite-based system. With ground tracking stations properly located on a common geodetic datum, the spatial positioning of the satellite could be well determined and monitored. The system should be global, although it might initially be biased in favor of certain areas of the globe. The geometry of the system could be optimized. It should be available continuously. The system should permit the user to come into an area at any time or position and obtain continuously an unambiguous, corrected, absolute position, directly related to the geodetic datum. Finally, it should have an output capable of high-precision measurement that could be sensed at grosser scales if high precision is not required. An aircraft or moving vessel at sea requires a lower positional accuracy than a stationary vessel or one sampling at a high rate in shallower waters. This should permit the production of a range of sensors (receivers) economically tailored to the needs of a wide spectrum of users.

This system would be limited in accuracy only by the basic geodetic datum determination and the current state of electronic technology. Both of these problems are receiving adequate attention at this time. The fact that most users of marine navigation systems are satisfied with considerably less accuracy should not be permitted to influence the design criteria. This system should be oriented to the highest requirements, which appear to be those of the exploration and surveying field.

For work on the continental shelf, high-precision short-range navigation will continue to be required. Adequate systems are now available, and their utilization is more a problem of organization and finance than of development. Certain of these systems can be mounted in buoys, which, if located on the worldwide geodetic datum, would permit relating detailed large-scale surveys to the general global coverage. Similarly, underwater acoustic transponders now permit accurate short-range navigation but must be related to the worldwide system.

PLATFORMS

In the design of an ocean-exploration program, one must consider the use of a wide variety of platforms, including ships, buoys, aircraft, satellites, submersibles, bottom-mounted facilities (including towers and manned habitations) and free devices (such as drones, free buoys, and capsules). Each platform will lend itself to a particular portion of the space–time spectrum, but no one platform will cover the entire spectrum in a simple or economical fashion. Platforms must be "tuned" to the mission they are to perform.

Some platforms are clearly more versatile than others. For example, the classical oceanographic platform, the research vessel, is capable of a wide variety of observations, particularly when carrying a small submersible. Yet a ship has many limitations, including its relatively slow speed and its inability to work in all weather or in ice-covered seas. A fixed data buoy, on the other hand, is a very specialized platform, capable of continuous measurement of certain variables, but unable to obtain any information on spatial variability. Based on a preliminary analysis, a high versatile platform appears to be the shallow (1,000 ft) support submarine with its own laboratories, submersibles, divers, and unmanned probes. No such submarine exists, although the general concept has been proposed.

During the initial years at least, programs of the Decade will be carried out on existing platforms. Present research vessels and those under construction will have to be augmented if the significant expansion of fieldwork we project is to take place. New vessels that would not be available until the mid-Decade may be substantially different from those now operating. For example, a "resident ship" operating in the South Pacific would require unusual speed and endurance to accomplish its mission, and some sort of surface-effect craft might turn out to be desirable. On the other hand, a suitably instrumented aircraft may turn out to be more cost-effective for some types of measurements (see below). More effective use of ships of opportunity is required; the necessary development is, however, more in the instrumentation of such ships than in the ships themselves.

In order to monitor variations in the ocean and atmosphere, moored buoys equipped with suitable sensors will be required, possibly in large numbers and widely distributed throughout the ocean. The prototype buoys now being tested lack reliability, accuracy, and versatility, and they are not yet ready for widespread use. Major technical problems in the design of both moorings and sensors have yet to be solved, but further development is being vigorously prosecuted, experimental arrays are being installed, and steady progress is being made. We consider that continuing and adequate support to the present national program of research, development, testing, and evaluation of data-buoy systems will provide invaluable assistance to many of the programs proposed for the Decade.

Increased use of aircraft and satellites is foreseen, in connection with both the Decade and proposed large-scale meteorological programs. It is now possible to obtain temperature information from below the ocean surface by means of expendable bathythermographs used from both helicopters and fixed-wing aircraft, the latter having long range and high payload. Such aircraft have the capability to both deploy and retrieve instrument packages from the sea, and they can also be used for in-flight observations. They also have extended capability for obtaining oceanographic data with the speed that is essential for certain studies that have been handicapped in the past by the slowness of ships. Instrumented aircraft are an essentital part of proposed air–sea interaction studies. Remote sensors on satellites already provide valuable information on cloud cover and the development of storms; early in the Decade, useful measurements from space of sea surface temperature and sea state should be available. The monitoring of surface chlorophyll concentrations may also become possible. High-resolution photographs from satellites have already yielded useful scientific information. As noted elsewhere, satellite navigation will have an extremely important role in the Decade, and satellites capable of locating and interrogating data platforms should be in routine operation.

Small research submersibles now in operation are characterized by high obsolescence and limited depth, endurance, and payload. As the recent loss of *Alvin* has demonstrated, the surface handling and transfer of such vessels remains a difficult problem. Submersibles of improved design are being rapidly developed, largely by private industry. Means must be found to provide qualified institutions with adequate funds for the chartering of commercial submersibles for use in the Decade and other appropriate programs. As noted above, emphasis should also be given to the design and construction of larger submersibles specifically intended for a work-and-support mission at moderate depths and with diver-transport capability.

Bottom-mounted facilities include towers and manned habitations. Towers will normally be installed for other purposes (petroleum drilling and exploitation, navigational aids, for example), and their use is somewhat analogous to that of ships of opportunity. Suitably instrumented, such installations are invaluable for studies of air–sea interaction and variability and for monitoring changes in the ocean and the atmosphere. Manned habitations on the sea floor permit investigators to make detailed studies of the environment in the vicinity of the habitat. With governmental and private programs in the United States and elsewhere, steady progress is being made in the development of technology and experience in habitats at ambient and one atmosphere pressure. Thus during the Decade, it is likely that important use will be made of this *in situ* capability. Accelerated technological and scientific advances could be expected if a major new goal, such as occupation of the Mid-Atlantic Ridge, were projected.

During the last few years, there has been increased use of free devices, such as cameras and current meters, that descend untethered from the ship and, upon completion of their task, return to the surface for retrieval. Self-contained instrument capsules have been devised that can measure deep-sea tides (by recording the fluctuating pressure on the deep-sea floor to an accuracy of a millimeter of water), temperature to a few millionths of a degree, and water currents in the range of 0.1 to 10 centimeters per second. Unmanned, self-propelled undersea probes, or drones, have been developed for the measurement of sound velocity and thermal and other physical properties of the ocean. The further development and use of such free devices may prove to be economically feasible and should greatly extend the capabilities of the research and survey vessels in service during the Decade.

Living-Resource Location and Extraction

Although commercial fishing has benefited in recent years from improved vessels and methods of capture, much of the world catch results from methods little different from those in use early in the present century. There would appear to be a great opportunity for improved technology, both to permit reduction of the unit cost of capture and to develop the capability for the economic exploitation of presently unused resources.

For the achievement of Decade goals, the development of improved methods of search and detection would be particularly useful. Such methods would not only benefit the commercial fisherman directly by reducing the unproductive time spent in scouting, but also would be extremely useful in the exploration and assessment of living resources. Present detection is mostly based on visual spotting and on active and passive sonars (echo sounders). Acoustical systems have a limited range, target identification is difficult, display methods present serious interpretation problems, and the devices are relatively ineffective in rough seas. Although visual spotting, especially from aircraft, has a somewhat greater range, it also has numerous limitations.

Possible developments of acoustic systems include the use of bottom-bounce sonar, towed sonar (from ships), dipping sonar (from helicopters), or instrumented drones to extend the range of detection. Passive systems could be developed to detect fish with sonic tags, to identify some species (by their sound patterns), or to estimate the biomass of some species (from their own noise). Development of such systems will require not only improved instrumentation, but also the cataloging and spectral analysis of biological sounds, the relation of noise to standing stock, and the study of fish target strength (both individual and school), including spectral reflection differences over a broad band of frequencies.

Apart from visual spotting, optical-detection methods may include use of high-resolution television, spectral photographic techniques, or infrared or bioluminescence detectors; any of these might be developed for use from airplanes or spacecraft. Consideration should also be given to pulsed laser systems for high-resolution detection at relatively short ranges.

Although no chemical detection systems are now used, it may be possible to develop techniques to sense organic vapors at the surface or to detect the organic residue left after the passage of a fish school. In the case of electromagnetic detection, surface schools may be seen by high-resolution radars; fish schools may even cause detectable electromagnetic disturbances.

New techniques for aggregation and capture of living resources must also be considered. It may prove possible to aggregate fish schools by using air-bubble curtains, electricity, light, or low-frequency or biological sounds. Although the development of such techniques falls outside the scope of the Decade, their success will depend on studies of fish behavior and the response of fish to various physical stimuli that may be included in some biological programs of the Decade.

Survey Methods

With improved methods, the hydrographic survey of the continental margins and the deep-sea floor could be made less expensive and time-consuming. Early emphasis must be given to the development of improved automated survey systems, possibly involving side-scanning sonars with significant horizontal range. Output might be digital and be coupled through shipboard computer systems to all required navigational and environmental information so that charts of the sea floor could be produced quickly and with minimum intervention from personnel.

Data Management

The activities of the Decade will lead to the production of large quantities of data. To be of maximum utility, these data should be of suitable and known quality, should be in computer-compatible form, and should be readily reducible by automatic methods. At the same time, modern techniques of data management will be required if the information is to be assimilated and made available promptly in suitable form to the scientific and engineering communities. The data management study being sponsored by the National Council on Marine Resources and Engineering Development should lead to important progress in this field.

The management of data to be utilized in real time presents special problems that are not treated here in detail. In general, these problems concern physical measurements of time-dependent variables that are closely related to similar measurements in the atmosphere. The development is foreseen of systems for monitoring changes in ocean circulation and characteristics and for using the resulting information in models for forecasting atmospheric and ocean conditions. Thus the management of oceanographic data in real time is linked to the analogous meteorological problem, and it is desirable that common systems of sensing, communication, and analysis be developed.

Here the more general problems of data-handling are considered. Pertinent matters include the following:

1. Quality of data introduced from data sources
2. Processing and analysis
3. Archiving and retrieval
4. Output and display
5. Exchange and communication among data centers

Comments on data standards are given separately.

Since the International Geophysical Year, an interlocking system of national, regional, and world data centers has developed. In this country, the activity is centralized in the National Oceanographic Data Center and the associated World Data Center (A) for Oceanography (an analogous center is located in Moscow). National data centers have been established in a number of countries, and regional centers are operated by the International Council for the Exploration of the Sea and by the Japanese Oceanographic Data Center (in support of the Cooperative Study of the Kuroshio and Adjacent Regions). In order to handle efficiently the anticipated new levels of oceanographic data acquisition and use resulting from Decade programs, both national and international components of the present system must be strengthened, modernized, and automated.

In the desired system, data would be obtained or transformed aboard ship into computer-compatible form. It should be the responsibility of the originator to carry out primary quality control, and the availability of shipboard computers will greatly facilitate this task. Data would then be promptly transmitted to a shore center where they would be submitted to such processing as is necessary for entry into the data bank. Submission of data in a standard format will facilitate this step. The storage system must be designed to make possible speedy retrieval on a variety of bases including location, time, and kind.

Data centers will be called upon more and more frequently for products requiring further processing and analysis. Drawing on its data bank, a center should be able to produce analyses to meet the requirements of its users. The end product may consist of tabulations, graphs, or charts. It should also become possible to display information in the data bank in the form of a "live atlas," produced by an automatic plotter or on the face of a cathode-ray tube, so that the user can explore the data available to him in the process of establishing the definitive presentation he prefers. The ultimate system should also incorporate remote consoles through which laboratories can have direct access to the central data bank.

The "live-atlas" concept is not restricted to examination of physical and chemical data from the water column. For example, it has been proposed that such a system be established for studies of bathymetry and of other structural and physical properties of the sea floor. The data bank could include data from echo soundings, seismic reflection and refraction profiles, records of the analysis of core and dredge samples, reproductions of bottom photographs and magnetic and gravity profiles, time-series records, and basic station data. To initiate such a system, it is necessary to prepare suitable software programs for production of charts of geographic distribution, correlation of properties, construction of profiles along geographic lines, and display of time series and statistical properties, among other things.

Not all ocean observations lead directly to digital data. For example, many biological and geological programs result in collection of samples that must be subjected to analysis before computer-compatible data (e.g., counts and concentrations) are available. In some cases, the analysis does not lead to such data. Provision must be made for the efficient handling of samples resulting from Decade programs. In the case of biological samples, for example, it will be desirable to expand and strengthen sorting centers like the one operated by the Smithsonian Institution.

It should be noted that each consumer will require that information be made available to him in a form peculiar to his needs. Thus the engineer or the biologist may require a display or product of certain physical data in quite a different form than that needed by the physical oceanographer. The data system should have the necessary flexibility to accommodate the needs of a wide variety of users.

A special effort will be required to ensure that permanently useful engineering data and information are extracted from Decade results and other sources, that these data are produced in a suitable format, that engineering data and information requirements are identified, and that efficient access to data and information is provided. The orientation of this effort should be toward the working ocean engineer and to ocean-resource development and utilization. Such an effort, if successful, could serve as a catalyst for stimulating progress in other Decade data management areas.

Internationally, the U.S. national data center will feed into regional or specialized data centers and into the world oceanographic data centers. These world centers have been little more than libraries of archived data, and it is essential that during the early years of the Decade, they be closely tied into the modern computer-based facilities of an advanced national center. This could be achieved by designating certain national centers as the supporting centers for international data banks.

Data Standards

During the Decade, data will arise from a great variety of sources—from various private and government laboratories in this country and abroad. A basic requirement of cooperative projects is that resulting data can be readily intercompared and pooled. Thus the problems of standards and intercalibration of methods are of critical importance for the Decade.

Solution of these problems will require research on improvement of methods, international consultation and agreement on reference methods and standards, and the ready availability of modern equipment and suitable standards. Methodological problems are being investigated in this and other countries, and international working groups exist for the consideration of specific methods (for example, zooplankton laboratory and sampling methods, the measurement of photosynthetic radiant energy, direct current measurement, and determination of salinity from conductivity). This work must be intensified if the goal of data intercomparability is to be achieved early in the Decade.

In order to further the development of improved instruments, the provision of standards, and the intercalibration of methods, consideration should be given to the establishment of a national calibration and standards facility to complement academic and industrial facilities. This could possibly evolve from the newly formed National Oceanographic

Instrumentation Center and the National Bureau of Standards. Such a facility could offer the following services:

1. Provision of instrument-testing and instrument-calibration services and procedures
2. Recommendation of standard or reference procedures for data acquisition
3. Provision of agreed data-quality standards
4. Investigations of improved methods and instruments

These services should be available at cost to scientists and engineers in private and governmental laboratories in this country and in other countries participating in the Decade.

NATIONAL PROGRAM MANAGEMENT

It is not a function of the present section to propose the specific details of program-management procedures, but rather to examine some elements or principles related to successful implementation of the Decade. The discussion is not concerned with the assignment of program responsibility to one agency or another or with any rearrangement of these agencies that may occur; it does assume that the principal initiative for organizing the United States participation in the Decade will lie with the U.S. Government and that in the National Council or some equivalent body there will be a central staff devoted to planning, coordination, and review of the national Decade program.

This is not to say that all, or even most, activities should be conducted by government agencies. Important roles will be played by scientists and engineers from state laboratories, private institutions, and industry, and means must be found to achieve the most effective integration of various interests.

Planning

The planning function includes the development of programs and budget, the allocation of tasks, and the establishment of priorities. This report is concerned with some aspects of program development, particularly from the viewpoint of the scientists and engineers consulted. Those who have a responsibility for planning the activities of government agencies are also engaged in developing programs germane to the concept and objectives of the Decade. Because of the pressure of time, we have not studied existing programs and proposals in any detail. It should be an early task for the central planning staff to relate the concepts and proposals contained herein to the on-going and planned programs of federal, state, and private agencies in order to ensure that Decade-relevant activities are clearly identified and integrated into an over-all program.

Further discussions with the ocean-science and ocean-engineering community would be useful in developing a broader viewpoint on possible Decade programs. During the present study, it was possible only to sample this group and to compile a general point of view along with a number of specific ideas from those consulted. The planning phases of the Decade will require several years, and there is still time to ensure that a broader segment of the ocean community will have an opportunity to comment and to contribute ideas. Circulation of the present report is certain to elicit vigorous response from scientists and engineers, especially those whose views are not adequately represented. The ocean committees of the two Academies should find means by which these suggestions can be introduced into U.S. planning for the Decade. A joint standing committee from the two parent committees could not only review and forward such suggestions, but could also actively solicit new views, particularly in areas not adequately covered in this report.

As noted early in this chapter, we have not made a detailed study of the budget necessary for implementation of the Decade, for we believe that this must await elaboration of program details by potential participants; however, some general comments on funding are presented at the beginning of this chapter. Nor have we considered the allocation of Decade responsibilities to one or another government agency, since this must await action by the Executive and Congress on the organizational recommendations of the Commission on Marine Science, Engineering and Resources.

Some preliminary thought has been given to the establishment of priorities. At whatever level the Decade is eventually funded, support will be finite, and a selection will have to be made from various proposals for Decade programs. It is also clear that, however skillful one is in establishing priorities on an objective basis, there will be occasions where nontechnical considerations related to national policy will have an overriding influence. Having recognized this, it is still necessary to consider criteria by which objective priority decisions can be made.

As a first approximation, we have judged the three principal discipline-related program areas (geology, biology, and physics) to be of about equal importance. The programs described in this report have been selected from a large number of possibilities—this selection constituting a first rough priority judgment. Within a program area, an order of priority can be assigned to the proposals once potential participants have elaborated them in detail. With further planning and more extensive consultation among the scientific and engineering communities, other programs worthy of support will emerge.

As a general principle, the intent of establishing priorities should be to eliminate less-desirable projects rather than to reduce support of all conceivable projects to fit the available resources. It is more desirable to conduct selected projects at an adequate funding level than to diffuse available support through a larger number of projects that cannot be implemented in suitable depth and quality. Weight should be given to the economies of time, money, and personnel that can be achieved by using common facilities and locations for more than one project.

Principles for assigning priorities for projects where no immediate application of the knowledge is foreseen are different from those where application of such knowledge is more obvious. In the former category, within a single program area, members of the profession should be able to order projects on the basis of their relative scientific importance once project details have been fully elaborated. Between program areas, such judgment is very difficult, and it may first be necessary to make such preliminary distribution of available resources among the various areas.

For projects where economic returns can be identified, the returns will usually be too distant in time to be susceptible to benefit-cost analysis. Given the decision to explore ocean resources as a facet of national (and international) policy, the criterion of potential economic value can be used in ordering projects and in establishing their relative value to society. In carrying out such an analysis, the economic consequences of technological change must be kept in mind.

The establishment of geographical priorities requires special comment. Possible criteria include the following:

1. Present knowledge: exploration is more important in unknown regions, whereas processes may be better studied where conditions are well known.
2. Jurisdiction: although resources are more available on the shelf, cooperative investigations run into fewer jurisdictional problems on the high seas.
3. Research interest: phenomena in some regions are more clearly defined and susceptible to understanding than in others.
4. Utilization: the abundance or demand, or both, for resources differs from place to place.

The interaction of the various criteria makes the assignment of general geographic priorities very difficult. For a given purpose, this may be possible, and commonality of purpose should be sought, but the chance that many regions will be assigned equally high priority from the several points of view is relatively small.

Coordination

The difficulty of coordinating cooperative ventures is a function of the complexity of the program and the variety of participants. Coordination of the U.S. components of the Decade must recognize that programs will be funded by federal, state, and private sources and will involve government, academic, and industrial participants. If one assumes that the major source of funds will be the U.S. Government, coordination of that major component can be effected by some appropriate interagency group, such as the National Council on Marine Resources and Engineering Development or its successor.

Mechanisms for coordination of U.S. Government activities with those of state or private organizations are less well developed. To meet this need for coordination, it will be necessary to create a new composite body with special responsibilities for coordination of the U.S. *national* (in contrast to the U.S. Government) component of the Decade program. Such a body should be established early in the planning period.

Review

A mechanism is required for review and updating of Decade programs from outside as well as from within the government. External review might be accomplished by the joint NAS-NAE committee proposed in the Planning section above, which could undertake the responsibility for the long-term task of augmenting, updating, and evaluating the scientific and engineering aspects of Decade programs. The existence of such a committee could ensure the continual and active interest and participation of ocean scientists and engineers.

A continuing function of monitoring and appraising the utilization achievements of the Decade must be recognized. After the first years of the Decade a continuing economic study could be undertaken to monitor the progress of activities. In a sense, this could be regarded as on-going benefit–cost analysis. The study could evaluate questions such as:

1. The amount and the specific form (time pattern) of the investments made in the different projects.
2. The success of each program in its own terms, i.e., the adequacy

of the scientific and engineering results obtained relative to the original definition of the project, and the cost behavior of the project throughout its life.

3. The technological changes that can be identified with the program as a whole and with specific projects. The source of such technological changes should be identified whenever possible, and their applicability in other areas should be investigated.

In addition, as noted earlier, studies should be undertaken during the Decade, to examine the amount, source, and rate of output of trained manpower. The study should be concerned with the questions of investment in human capital and the returns from this investment. Other socioeconomic aspects of the progress of the Decade and its impact on society should also be monitored. Nationally, these studies might be a part of the planning responsibilities of the central coordinating body, or they might (better) be carried out by a more independent and objective group. A parallel international activity is indicated later.

ASSOCIATED SOCIAL STUDIES

The Decade has the objective of achieving more effective utilization of the ocean and its resources; several of the goals are specifically concerned with increasing the net yield from the use of marine resources. The scientific and engineering programs proposed for the Decade are intended to aid in removing the technical impediments to enhanced utilization. At the same time, it must be recognized that the objective and goals of the Decade cannot be achieved by technical means alone. There is a parallel set of problems in fields such as law, economics, political science, and sociology. In view of these problems, we propose that pertinent social studies be initiated and conducted as an integral part of Decade planning and implementation.

A distinction should be made between these studies and the application of the tools of the social sciences, such as benefit-cost analysis, in the planning, management, and monitoring of the Decade. Suggestions for the use of such tools are made elsewhere in the report.

Several examples of projects that might be appropriate are included here:

1. Development of theoretical models of joint maximization for application to international exploitation of fish stocks. For a given fish stock and set of exploiters, the models must analyze the appropriate

inputs and the relevant ranges for the division of the net product of the process of exploitation under conditions of different levels of income, different tastes, varying levels of uncertainty, and other relevant factors.

2. Studies of individual fisheries to quantify the potential gains (rent from the resource) in terms of returns to capital, labor, and governments (from taxes and license fees) that would result from a move away from the present common-property, open-access fishery toward a unitized fishery.

3. Studies of social costs, benefits, and rates of return for marine mineral-development projects. The analysis would pay particular attention to the generally neglected external costs and benefits—the so-called spillover effects.

4. Studies to develop a basis for construction of economically sound leasing, licensing, and taxing policies that will assist in the development of ocean petroleum and mineral resources while protecting the general welfare at the national and international levels.

STRENGTHENING POSSIBILITIES FOR INTERNATIONAL COOPERATION

The possibilities for international cooperation depend on several factors, two of which are discussed here. Many coastal states (nations) that could contribute to, and benefit from, programs of the Decade have not yet achieved the capability for meaningful participation. The need for assisting in development of this capability is discussed below under the heading "Mutual Assistance."

Success of many Decade programs will depend on the freedom to conduct scientific research everywhere in the world ocean. This freedom is affected by the manner in which coastal states define and administer their jurisdiction over the waters and the sea floor adjacent to their coasts. This issue is discussed later under the heading "Freedom of Scientific Research."

Mutual Assistance

Many countries bordering the sea lack trained manpower and facilities necessary for investigation of the nearby areas of the ocean. The more developed countries, in their studies of the ocean and its resources, will need the assistance of these coastal countries, not only in obtaining permission to extend their investigations into the coastal zone (see the following section) but also in carrying out the continuing observations necessary for monitoring local changes as they relate to conditions in a broader region. The coastal nation can benefit directly from an increased knowledge of local resources, but needs to acquire the capability to interpret and apply resource information and to make rational decisions on investment and management of such resources. An important part of the Decade should be a mutual assistance effort directed toward strengthening the potential of interested coastal states in marine science and its applications.

In developing countries that have limited resources, principal emphasis will undoubtedly be given to studies directly related to the development of marine resources. Such studies depend on a base of fundamental research that could be conducted cooperatively with investigators from other countries in the region or from more-developed countries outside of the region.

The development of a successful national program seems to depend on a number of factors, including commitment of the government to continuing support, development of a coordinated program that integrates all available and proposed efforts into a concerted attack on marine problems of interest, and establishment of career opportunities to attract and retain competent young people in the field. With these conditions met, the problem is to train a sufficient number of specialists and to provide them with the necessary facilities and equipment with which to work.

A number of mechanisms already exist for providing technical assistance in the marine field to developing countries. These include bilateral and multilateral (regional) arrangements among governments as well as the more widely international programs administered by the Development Program and the several specialized agencies of the United Nations. In addition, there are nongovernmental agreements among commercial firms of various nations. It is important that the United States recognize the importance of enhanced mutual assistance to the achievement of the objectives of the Decade and the opportunity presented by the Decade for a more concerted approach to the development of national capabilities in marine science and its applications. This recognition should be reflected in the planning of Decade programs and in the allocation of financial resources for activities associated with these programs.

Freedom of Scientific Research

The need for broad cooperation in the study of such a vast region as the world ocean is reflected in the long history of successful joint oceanographic endeavors. Ships of many nations sail the ocean freely, yet jurisdictional conflicts among these nations can easily occur.

The Decade will bring an increased emphasis on the continental margins of the ocean and the overlying waters, where resources are most accessible to man. Yet it is precisely in these regions where international cooperation may encounter some difficult restrictions. With the recent extension of coastal sovereign rights over the natural resources of the continental shelf, certain scientific research on the shelf now requires the consent of the coastal state. Fishery research may also be subject to legal restrictions as a result of recent expansions of coastal jurisdiction beyond the territorial sea and extensions of the latter area.

To facilitate successful implementation of Decade programs, it would be useful if participating nations were to recognize that the scientific research portion of exploration and reconnaissance is an integral part of fundamental scientific research. Convenient procedures should be established for facilitating cooperative research on the continental shelf, in the contiguous fishing zone, and in the territorial sea. These procedures should reflect the mutual benefits to the parties concerned, and they should ensure that the coastal state can actively and fully participate in the specific program concerned, that resulting data will be made promptly available and samples readily accessible to the coastal state, and that the results of such investigations will be openly published in timely fashion.

In addition to removing jurisdictional impediments to free scientific research, nations participating in the Decade must find ways to remove barriers to the free exchange of personnel and data and to facilitate the passage of scientific materials through customs and the entry of research vessels into their ports. Valuable experience along these lines has already been gained during the International Indian Ocean Expedition and other cooperative investigations developed under international auspices.

INTERNATIONAL COORDINATION

The International Decade will consist largely of an integrated set of national programs. These can be coordinated both through bilateral arrangements and by multilateral (regional) or more broadly international mechanisms; there are international bodies, both nongovernmental and intergovernmental, that can be used for this purpose.

Nongovernmental scientific, engineering, and technical organizations can contribute to review, planning, and evaluation of the technical content of Decade programs. Such organizations can also examine methodological problems, such as those of standardization and intercalibration, and can recommend research or other steps required for their solution.

Implementation of national programs contributing to the Decade will be viewed internationally as a governmental responsibility. Intergovernmental organizations are required to facilitate governmental agreements on arrangements for concerted action. In addition, these organizations may have their own programs that support objectives of the Decade.

In this section, the tasks to be accomplished by the coordinating apparatus are first discussed; then, the suitability of existing bodies to undertake these tasks, and the relation of the Decade to other international cooperative activities, are examined.

Coordination Tasks

Tasks to be accomplished in coordination of an international program such as the Decade differ from stage to stage.

In the planning stage, before the start of fieldwork, it is necessary to review and synthesize available information on the region or problem; the results of this review can be discussed internationally by potential participants. Agreement is needed on objectives and on methods for the investigation of identified problems. Over-all objectives, including those of a technical nature, will be established by governments. Through intergovernmental discussions, agreement can be reached on the nature and extent of national participation. On the basis of detailed program recommendations from each participant, the integration of national programs can be worked out.

During the fieldwork, coordination depends, to a large extent, on good communications and full exchange of information. Both technical and operational information must be made available to other participants and to the international coordinating office so that prompt decisions can be made on the basis of full knowledge of the situation. During this stage, the international coordinator may be required to solve various operational problems that depend on the cooperation of several participants.

Two kinds of monitoring and review are desirable during and after the operational stage. First, the results of observations must be continually evaluated so that the program can be updated and the need for additional or different observations can be recognized and made known; new scientific and technological advances should be incorporated as they are developed. Second, the economic and social consequences of Decade activities should be kept under review. The Decade is intended to accelerate technological change in the use of the ocean and its resources. To maximize the benefits of Decade programs, progress in achieving this objective should be appraised and made known to relevant decision-making bodies.

At the conclusion of the field program, coordination and joint action continue to be required in the exchange of data, samples, and other results; in the pooling and joint analysis of these results, the preparation of syntheses and interpretations for application to scientific or utilization problems, and the publication and dissemination of atlases, reports, and other products of the program.

Existing Organizations

Much of the technical planning and review referred to above can be undertaken by groups or combinations or groups already acting as advisers to intergovernmental bodies. Thus, a joint working party has already been established by the Scientific Committee on Oceanic Research [SCOR: advisory to the Intergovernmental Oceanographic Commission (IOC) and UNESCO], the Advisory Committee on Marine Resources Research [ACMRR: advisory to IOC and Food and Agriculture Organization (FAO)], and by the World Meteorological Organization (WMO) itself, to examine in a preliminary way the scientific content of an expanded program of international cooperation. Because these organizations have ready access to appropriate scientists, they could be asked to establish advisory panels for the necessary continuing scientific review and evaluation of Decade programs.

However, the groups presently providing advice do not adequately represent all aspects of ocean affairs pertinent to the Decade. There is no adequate mechanism for providing engineering review and evaluation of international ocean programs or for involving representatives of industry. The social scientists are in no way represented. There exist a number of international nongovernmental engineering-related groups with interest in the marine environment.* Possibly such groups could jointly strengthen and coordinate their efforts in ocean affairs and be available to provide engineering advice. Alternatively, a strengthening and consolidation of existing scientific bodies, with established advisory links to some intergovernmental organizations, could be accompanied by a broadening of scope to become a joint scientific and engineering body concerned with the ocean. Representative social scientists could be invited by the aforementioned advisory groups to assist in the monitoring and review of economic and social consequences of the Decade as it unfolds.

From the intergovernmental point of view, there are several existing

* For example, the International Association for Hydraulic Research (IAHR), the International Association for Water Pollution Research (IAWPR), the International Organization for Standardization (ISO), the International Ship Structures Congress (ISSC), the International Society for Soil Mechanics and Foundation Engineering (ISSMFE), and national engineering societies with members from other nations.

organizations whose responsibilities would cause them to be associated with the Decade. In December 1968, the United Nations General Assembly welcomed the concept of an International Decade of Ocean Exploration and requested the Intergovernmental Oceanographic Commission to develop and coordinate the scientific aspects of the program. This should be done in close cooperation with the Fishery Department of the Food and Agriculture Organization, and the WMO. These organizations in their present form may not be able effectively to organize and implement a program of the large scope of the Decade, including as it does both scientific and engineering aspects. However, some consolidation or restructuring may occur by early in the Decade; in the meantime, the IOC is functioning as the "lead agency" in the preliminary phase of developing the program.

The Commission has already taken steps to fulfill this role. Consultations have been undertaken with FAO and WMO to develop a broadened IOC structure in which these organizations can more effectively participate. Member states have been invited to submit their views on an expanded program, and an IOC working group has been established for planning purposes. The Commission's considerable experience in the coordination of international cooperative investigations can be usefully applied to organization of the Decade.

Even if there were a more effective integration of IOC, FAO, WMO, and other intergovernmental agencies' activities than now exists or seems likely in the near future, important aspects of the International Decade will be developed on a regional or more local basis. The organization of coordination in the bilateral case offers no particular difficulties, as the necessary procedures can be incorporated in the basic agreements. There exist regional intergovernmental organizations that could be utilized for coordination of regional Decade programs. The most effective of these are in technically advanced regions (such as ICES in the North Atlantic) or have much broader interests than the ocean (for example, OECD). There is limited experience, to date, in coordinating effective cooperative marine investigations in regions consisting largely of developing countries. It is in these regions where the socioeconomic impact of Decade programs could be very large and where, therefore, particular attention must be given to developing better machinery for joint action.

Relation to Other Programs

Cooperation in the study of the ocean and its resources has a long history, dictated by the large scale and dynamic complexity of this environment. Particularly noteworthy are the cooperative expeditions developed during the International Geophysical Year and as a part of the International Indian Ocean Expedition. Such ventures are already being developed for the future and will continue to be developed whether there is an International Decade of Ocean Exploration or not.

For example, the IOC has initiated a program known as the Cooperative Investigations of the Caribbean and Adjacent Regions, to be implemented in the early part of the 1970's. Other IOC programs are being developed in the Mediterranean, North Atlantic, and Southern Ocean. The Integrated Global Ocean Station System (IGOSS) being developed by IOC will require cooperative studies as well as the elaboration of an operational monitoring network. This program is related to activities of the WMO, namely the World Weather Watch and the related Global Atmospheric Research Program (GARP), consisting of cooperative studies, primarily meteorological but with important oceanographic aspects.

Cooperative fishery investigations are also being organized by bodies related to FAO, often with the support of the United Nations Development Program (UNDP), or by independent international fishery organizations. Predevelopment fishery surveys jointly funded by the UNDP and the recipient nation, the expanded technical assistance program in fisheries, and the regular program of the FAO Fishery Department now amount to about $25 million per year and affect more than forty developing countries. These programs can be expected to grow during the Decade.

To what extent should such cooperative programs be considered a part of the Decade? In most cases, the goals of these programs are close to those of the Decade; given the existence of a Decade, some of them might have been developed within its framework. But they have grown before the Decade and its implemental mechanisms have been established. As these mechanisms come into existence, it is likely that certain programs whose achievement will be facilitated by such action will be absorbed and coordinated within the Decade. This is probable, for example, in the case of cooperative investigations of IOC. Other programs, such as GARP and the fishery development projects, will undoubtedly continue to be developed outside of the IDOE, so there will be a continuing need for close coordination of the IDOE and other cooperative activities in the marine environment. Although it may be possible to make a logical determination in each case of whether a program should or should not be incorporated in the Decade, in practice, the decision should be based on the most effective means of implementing the program under the conditions prevailing at the time. If, as is likely, the IDOE will not comprehend all cooperative studies of the ocean and its resources, Decade-relevant programs should be identified and monitored in the same way as proposed for Decade activities.

APPENDIX

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